AN
INQUIRY
INTO
THE NATURE AND CAUSES OF
FEVER;

WITH

A REVIEW OF THE SEVERAL OPINIONS
CONCERNING ITS PROXIMATE CAUSE,
AS ADVANCED BY DIFFERENT AUTHORS;

AND PARTICULARLY

AS DELIVERED FROM THE PRACTICAL CHAIR IN
THE UNIVERSITY OF EDINBURGH.

INCLUDING

Some OBSERVATIONS on the Exiſtence of
PUTREFACTION in the LIVING BODY,

AND THE

Proper Method of CURE to be Purſued in FEVER.

EDINBURGH:
Printed for C. ELLIOT, EDINBURGH; and
G. ROBINSON, LONDON.

M,DCC,LXXXV.

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1. Mk. 38.

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AN
INQUIRY
INTO THE
NATURE OF FEVER.

INTRODUCTION.

THE diſeaſes afflicting mankind are in themſelves numerous, and withal ſo complicated, that our utmoſt endeavours to aſcertain the truth has fallen far ſhort of our anxious wiſhes, or the theory founded to explain the phenomena of diſeaſe has been oppoſed for the moſt part by ſome very powerful facts. A ſurvey of the ſeveral hypotheſes, which have prevailed ſince the earlieſt ages of phyſic, affords us a melancholy proof of the aſſertion; and in particular with reſpect to fever, many doctrines have at times gained countenance, which at laſt have ſunk into diſrepute, partly from their own inſufficiency, and partly from the greater plauſibility of a later ſyſtem.

To eſtabliſh a general principle, allowed by pathological writers, in our inquiries, was the motive that actuated me in the preſent attempt. The principle I allude to, is the foundation of the proximate cauſe of diſeaſes from the knowledge of the remote; and from a thorough inveſtigation of the latter, we may, I apprehend, generally pronounce what the nature of a diſeaſe is, and what remedies will be properly adapted for the removal of it. I have the authority of Dr Cullen for maintaining, that the cure of diſeaſes is to be obtained from the knowledge of their proximate cauſes; and he obſerves, that this laſt requires a knowledge of the Inſtitutions of Medicine, or an acquaintance with the ſtructure, action, and functions of the body in a ſound ſtate; of the change it is capable of undergoing; and of the powers which operate in producing this.

It is not preſuming on the knowledge we have attained in phyſic, when I aſſert, that from the extenſive obſervations already made by authors at different periods, we are not only acquainted with the ſtructure of an animal, at leaſt ſo much as is connected with the practice of phyſic; but likewiſe are verſed in many particulars concerning the operation of cauſes, and which we know will infallibly induce diſeaſe. In the preſent caſe, then, from the nature and effect of the remote cauſes operating on the ſyſtem being well aſcertained, it is hoped that the proximate cauſe of fever, as deduced from theſe, will not only be ſatisfactory in explaining the hiſtory of the diſeaſe, but when extended to practice will evince more fully the juſtneſs of my doctrine.

The reader, when he advances in his peruſal to the proximate cauſe, will readily remark, that I have not dwelt particularly on the refutation of the three firſt ſyſtems taken notice of, as being very much out of repute; and my attention is chiefly directed to the conſideration of the ſpaſmodic doctrine, now ſo much admired as being the lateſt, and which is ſo ably ſupported by that ingenious profeſſor Dr Cullen: for though I can by no means agree in opinion with him, yet I muſt do him the juſtice to ſay, his deſcriptions together with his arrangement of the phenomena of diſeaſes are by far the beſt that has been hitherto publiſhed, while his rejection of the humoral pathology in very many caſes does him infinite honour.

It may be thought in many parts of this treatiſe, I have been too tedious in reciting quotations from authors; but I muſt obſerve, that it is only in particular places, I have been ſtudious to derive authorities, where I have wanted to condemn the pernicious effects of any mode of treatment, or to repreſent the good conſequences from following another. I muſt however further remark, that I have ever been cautious to ſupport my ſeveral opinions by facts from the moſt reſpected authors; and if my reaſoning ſhould not be admitted, I have this conſolation left, that my authorities, on which this is built in ſome meaſure, muſt likewiſe be rejected. At the ſame time, I believe, the great loſs of moſt medical opinions is the want of ſuch proper vouchers. Advanced as they are, without ſufficient authority, and ſupported only by ingenuity, it is impoſſible, under theſe circumſtances, they can remain long unſhaken.

I cannot conclude theſe few lines of introduction without expreſſing my acknowledgements to Drs Lind, Donald Monro, Clark, Millar, and Milman, for many valuable facts and much information derived from the peruſal of their works; and I am happy to add, my quotations are chiefly taken from ſuch authors, whoſe names prefixed to any point of reaſoning will always enforce greater conviction.

CHAPTER I.

CHARACTER OF FEVERS.

DOCTOR CULLEN, in his Syſtem of Noſology, has eſtabliſhed a claſs of PYREXIÆ comprehending five orders. He

defines the claſs of Pyrexiæ very properly, thus: ‘ POST HORROREM PULSUS FRE-
‘ QUENS, CALOR MAJOR, PLURES FUNC-
‘ TIONES LÆSÆ, VIRIBUS PRÆSERTIM AR-
‘ TUUM IMMINUTIS.’—The firſt order of this claſs is the only one that comes under our inveſtigation, viz. Fevers.

The ancients conſidered heat as a fundamental diagnoſtic in marking fever It undoubtedly is one characteriſtic of the exiſtence of Pyrexia; but it does not imply the eſſential difference between the claſs and the order; and therefore, without the aſſiſtance of other circumſtances, our idea of fever would be but very incomplete.

BOERHAAVE and others conceived, that quickneſs of pulſe was a certain ſign of fever, and by it they were regulated as to the preſence of the diſeaſe. That a quickneſs of pulſe takes place in moſt caſes, there can be little doubt: but there are caſes where the pulſe does not exceed the natural ſtandard in its frequency of pulſations; in which inſtances, therefore, provided we had no other more certain diagnoſtic of fever, we ſhould more than probably pronounce the non-exiſtence of the diſeaſe when it was actually preſent.

I ſhall therefore adopt Dr Cullen's definition, as being far more accurate than any other, and which gives us at the ſame time a very ſufficient idea of the diſeaſe: 'PRÆGRESSIS LANGUORE, LASSITUDINE, ET 'ALIIS DEBILITATIS SIGNIS, PYREXIA SINE 'MORBO LOCALI PRIMARIO.'

CHAPTER II.

DIVISION OF FEVERS.

AS fevers frequently change their type, according to climate, method of treatment, obſtinacy of the diſeaſe, &c. I am inclined to believe that moſt fevers are eſſentially the ſame. I would not, however, with Dr Clark, affirm, there is no inflammatory fever independent of local affection *: for ſurely the

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* The inſtances of inflammatory fever, independent of local affection, are, however, very few.

fever that precedes the eruption of the diſtinct ſmall-pox, when the perſon has not been inoculated, is undoubtedly a ſynocha or inflammatory fever; and yet at this period no local affection whatever can be demonſtrated: therefore I think we are ſufficiently entitled to conſider it as an inflammatory fever, and not as a ſtate of pyrexia in conſequence of local affection.

The moſt ſimple, and, in my opinion, moſt obvious diviſion of fevers, is into Continual, Remitting, and Intermitting *. The firſt of theſe has no manifeſt intermiſſion; the ſecond remits, or has irregular and imperfect intermiſſions; and the laſt has diſtinct intermiſſions, during which time the patient is left nearly in a ſtate of apyrexia.

Of the ſpecies of continued fevers, it is to be obſerved, that as inflammatory diſeaſes are of the continued type, and as a ſyſtem

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* The various diſtinctions of fevers, into putrid, non-putrid, malignant, &c. that have been at different times ſpoken of, appear, in my opinion, very abſtruſe, and are not attended with any material advantage.

ſyſtem of noſology ſhould lead us to the method of cure in diſeaſes, ſo we would expect ſuch an arrangement as to accompliſh this great and important end. To this purpoſe it may be remarked, as ſynocha and typhus are fevers of the continued form, and the method of cure in theſe diſeaſes ſo diametrically oppoſite, I am of opinion a ſubdiviſion of continued fevers ſhould be made; the one to comprehend ſynocha, and the other typhus and its varieties. Reſpecting remittents and intermittents, we can be at no ſuch loſs; and therefore any further diviſion of them would only tend to perplex the memory, without any material advantage accruing from it. It is to be, however, attended to, that in this treatiſe I am only to ſpeak of thoſe fevers whoſe remote are debilitating cauſes, or, in other words, where a ſtate of debility is induced in the ſyſtem.

CHAPTER III.

OF THE PHENOMENA OF FEVERS.

THE following are the chief phenomena that preſent on the attack of fever. The patient complains firſt of a pain in the back and limbs, with conſiderable weakneſs in the joints, eſpecially in the knees, and it is with difficulty they can ſupport his body: Theſe ſymptoms continue generally, though not in all caſes, for ſome time before the acceſſion of the cold fit, when it is uſhered in with a ſlight chillneſs: the patient finds himſelf extremely averſe to every kind of exerciſe, being frequently diſpoſed to yawn and ſtretch his body, feeling altogether as if he was much fatigued and inclined to reſt: at the ſame time the features are much diminiſhed; and in the eye there appears a peculiar dulneſs and heavineſs, expreſſing very ſtrongly the languor and debility the patient labours under; while in ſome more dangerous caſes of typhus, the eye has been known to be red and inflamed as in ophthalmia, from the very firſt attack: The ſurface of the body becomes likewiſe greatly changed. As the cold ſtage advances, the ſkin is found pale and ſhrivelled, diſcovering an appearance of conſtriction in the veſſels; and it has been obſerved the nails turn livid and wan, ſo that many perſons know when the fit is coming on by an attention to this circumſtance. Theſe phenomena remaining for ſome time, advance to that degree, when the ſhivering is very remarkable, inaſmuch that the lower jaw is involuntarily moved upwards and downwards, occaſioning what is called a chattering of the teeth: the patient ſeeks external heat, and is happy when the rigors are by ſuch means alleviated. After this the ſhivering gradually leaves him, and a heat greater than uſual enſues: this generally comes on at firſt by frequent fluſhings of the face, which alternate with a chillneſs, till at laſt the heat becomes permanent; the ſkin now regains its colour, and acquires an additional redneſs; while the features aſſume a fulneſs, and are even ſo turgid as to appear ſomewhat ſwelled and inflamed. At the ſame time the ſuperficial veins appear diſtended, which before this period were not eaſily diſcovered. As the ſymptoms of the hot ſtage come on, a conſiderable pain is felt above the eye-brows ſhooting along the temples. This, however, does not always take place; but more or leſs of an excruciating ſenſation is conſtantly experienced near the temples; which, with the beating of the temporal arteries, obliges the patient to toſs and tumble in bed, in hopes of procuring an alleviation of his agony. After the duration of theſe ſymptoms for a longer or ſhorter time, a moiſture is felt upon the face and neck, which by degrees comes to be diffuſed in ſome meaſure over the whole body; but the extremities are never ſo moiſt as thoſe parts which are neareſt the heart, and hence liable to be affected in a more powerful manner by its influence. When the ſweating appears, the heat of the body is diminiſhed; the pain of the head, and the throbbing of the temporal arteries abate, which is more relieved as the ſweating increaſes, till at laſt the patient is left in a ſtate of apyrexia. But I would not with Dr Cullen aſſert, that the body is reſtored to its uſual ſtate, and that its functions are not impaired: on the contrary, I would allege, and in this I am ſupported by the united teſtimony of many reſpectable authors, there are marks of a general laſſitude and debility; and for the moſt part the patient is affected with a pain of the head, with a loſs of appetite; and in ſhort, there is ſuch a want of activity in the whole ſyſtem, as ſufficiently evinces that upon this occaſion the energy of the brain continues ſtill much diminiſhed.

DURING the ſeveral ſtages deſcribed, a conſiderable alteration takes place in many other functions of the body. On the approach of the cold fit, the pulſe turns quick, feeble, and not unfrequently intermits. As the hot ſtage advances, the pulſe becomes fuller, ſtronger, and the pulſations more equal; but when the moiſture of the body enſues, the pulſe turns ſofter, and continues full till the ſweating ceaſes to flow, when it returns more to the natural ſtandard, ſtill retaining, however, a quickneſs, and want of its uſual momentum.

Reſpiration alſo becomes frequent, ſhort, and difficult; and a dry cough often accompanies it, with repeated ſighs. A pain, too, is generally felt in ſome part of the thorax, moſtly in the one ſide. A ſenſe of oppreſſion is alſo experienced about the præcordia, which continues till the end of the paroxyſm; but this, with the laborious reſpiration, ceaſes when the ſweat has flowed for ſome time.

A conſiderable deviation in the proper exerciſe of the natural functions, is likewiſe obſervable. On the acceſſion of the cold fit, the patient is ſeized with a total averſion to animal food, the very ſmell of which occaſions a ſqueamiſhneſs and kind of rotatory motion in the ſtomach antecedent to vomiting: He is, however, extremely deſirous of fruit, and eſpecially that of an aceſcent nature. When the cold fit has continued ſome time, the nauſea that before exiſted is now ſo much increaſed, as to occaſion violent retchings: theſe are ſo extremely diſtreſſing to the patient, that he becomes faintiſh, and feels as it were a ſenſe of ſhrinking of the internal parts of his body, eſpecially about the ſtomach and right hypochondrium. At laſt theſe violent retchings end in vomiting: the matter evacuated is generally of a bilious nature; but whatever is lodged in the ſtomach comes off with the bile, and moſtly before it. In ſome caſes of fever that occur, in the warmer climates more particularly, there happens from the beginning of the diſeaſe ſuch a terrible vomiting, accompanied with an intolerable pain at the ſtomach, which at the ſame time feels hard, tenſe, and inflated, that the phyſician is often in danger of loſing his patients in the courſe of a very ſhort time.

A remarkable thirſt prevails during the whole paroxyſm. The tongue is ſometimes whitiſh; yet this does not take place in moſt caſes, till the diſeaſe has exiſted for ſome time; but when the tongue is dry and ſomewhat parched from the firſt attack, as is ſometimes the caſe in the jail-fever, it in-

indicates a more violent diſeaſe. There is always, however, from the beginning, a want of the ſecretion of ſaliva in the mouth, and the tongue is commonly impreſſed with a bitter taſte.

RESPECTING the other excretions of urine and ſtool, the former ſuffers a conſiderable change. From the beginning of the cold till the end of the hot ſtage, the urine appears conſtantly of a more or leſs red colour, which increaſes as the hot ſtage advances, till the ſweat has flowed for ſome time, when it depoſites a ſediment, previous to which it was clear, and voided only in ſmall quantity. With regard to ſtools, a coſtiveneſs prevails throughout the whole paroxyſm, except in ſome rare caſes, as has been alleged by Dr Cullen, where a diarrhœa attends from the commencement of the diſeaſe till its termination. He has remarked, too, that analogous to theſe alterations which happen in the ſtate of the ſecretions, ‘ tumours ſubſiſting on the ſur‑‘ face of the body, ſuffer, during the cold ‘ ſtage of fevers, a ſudden and conſiderable 'detumeſcence; but generally, though not 'always, the tumours return to their for'mer ſize during the ſweating ſtage. In 'like manner, ulcers are ſometimes dried 'up during the cold ſtage, and return again 'to diſcharge matter during the ſweating 'ſtage, or after the paroxyſm is over *.'

Early in the cold ſtage it is often to be remarked, that the patient becomes exceedingly dejected, fretful, and peeviſh; and this is the more obſervable when he is rouſed from the languor and drowſineſs which particularly attend the attack of the cold fit. On the beginning of the hot fit this drowſineſs leaves him, when a watchfulneſs takes place, and he then becomes extremely impatient of noiſe. A delirium † ſeldom occurs in the cold ſtage: it nevertheleſs frequently comes on in the hot, preceded by a noiſe in the ears; but when the ſweat begins to appear, this gradually abates till the paroxſym is over, being then gene-

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* Vide Cullen's Firſt Lines.

† Dr Lind, in his Hot Climates, gives us ſome inſtances where patients have been at once ſeized with a delirium and fever without any of the preceding ſymptoms we have deſcribed.

rally relieved of it: there ſtill remains however ſome confuſion of the head, which is more remarkable in the fevers of the warmer countries, and more eſpecially as the fever partakes of the continued type.

Theſe are the principal phenomena to be obſerved on the attack of fever, and I have endeavoured to point out ſome peculiarities which occur in a few particular caſes. However, upon the whole, I preſume that the phenomena I have marked, will be ſufficient to convey a proper idea of fever, as I have attempted to apply moſt of the ſymptoms to thoſe fevers which intermit as well as to thoſe that aſſume a continued form.

There are certain circumſtances, notwithſtanding, that attend ſome ſpecies of fever, which are wanting in others; and of theſe it might be imagined I ſhould have taken notice: but as my view was only to lay down thoſe that conſtantly accompany the attack of almoſt every fever, I conceived it more requiſite to mark them; and this the rather, as ſuch peculiarities do not happen in general, till the diſeaſe has exiſted for ſome time, to enumerate which is foreign to my intention.

The phenomena that have now been deſcribed conſtitute what is called the Paroxſym of a fever; and it ſometimes happens that a fever conſiſts of only one paroxſym, termed Ephemera, or fever of one day's continuance. But it more frequently occurs, that a fever conſiſts in a number of paroxyſms; the period, however, from the end of the one to the beginning of another paroxyſm differs in different forms of fever. Dr Cullen obſerves, he never ſaw in a long courſe of practice any ſuch fever as has been termed continent, or fever purſuing its carreer without any manifeſt intermiſſion or remiſſion. I am notwithſtanding diſpoſed to believe, that though every fever in the beginning ſhows marks of remiſſion, yet when it has conſiderably advanced, and the ſituation of the patient becomes highly dangerous, I never could obſerve the ſlighteſt alleviation of the ſymptoms; and I think in ſuch caſes the term Continent is by no means improper: for the patient has continually ſubſultus tendinum, with frequent pickings at the bed-cloaths, and lies on his back comatoſe for two, three, or more days before death; during which time, in many caſes that I have ſeen, there was no remiſſion whatever; and the diſeaſe at that period ſeemed to be a continuation of ſymptoms, which, as it hurried on to its fatal iſſue, became more and more aggravating, till death at laſt cloſed the ſcene.

CHAP. IV.

OF THE REMOTE CAUSES OF FEVER.

AS the phenomena of every affection to which the human body is liable, depend upon the operation of the remote cauſes inducing it; ſo it is evident, a knowledge of theſe enables us to judge with more certainty of the nature of the diſeaſe, than what we could poſſibly entertain provided we had no ſatisfactory account of the remote cauſes; and which, though even we endeavour to aſcertain to the utmoſt, our reſearches are in many caſes attended with very little ſucceſs. However, in the diſeaſe now treated of, from its very frequent occurrence, and from the accurate obſervations made by many different and reſpectable authors, no ſuch difficulty ariſes; and it is therefore preſumed, that the cauſes to be aſſigned will ſufficiently account for the phenomena which have been already deſcribed. I ſhall divide the remote cauſes of fever into two general heads; and this I conſider as requiſite, on account of the oppoſite nature of the ſeveral powers which induce it, and, from a general though vague manner of ſpeaking, by which all the remote cauſes of fever have been conſidered in the light of direct ſedatives. The firſt diviſion comprehends thoſe that directly debilitate the ſyſtem; as Contagion, Miaſma, Cold, the Depreſſing Paſſions, and Hemorrhagies from various parts of the body. The other diviſion has within its view thoſe that indirectly weaken the ſyſtem; as Exceſs in Venery, Frequent Intoxication, &c.

CONTAGION and MIASMA have been noted as the moſt common remote cauſes of fever. The former has been ſuppoſed to be of many varieties, inducing particular diſeaſes according to the nature of the infection. I am much inclined to believe, that the contagious principle acting upon the bodies of perſons, produces its effects according to the nature of the conſtitution to which it is applied, and again to the degree of concentration it poſſeſſes. In oppoſite habits, it is probable, that a very different effect will follow the expoſure of the body to a contagious diſeaſe: for example, two perſons of different conſtitutions ſhall be expoſed to the contagion of the ſmall-pox; and though they both take the diſeaſe, yet I apprehend that the one may have the diſtinct, while the other has the confluent kind. Nay, there are inſtances where perſons have been ſeized with a typhus in conſequence of being expoſed to the contagion of the ſmall-pox*; and on this account the effect of any contagious principle, it would ſeem, is to be attributed primarily to its concentration, and ſecondly to the nature of the conſtitution. It muſt be ſtill, however, admitted, that there is ſomething peculiar in the nature of contagion which determines it moſt generally to produce the ſame diſeaſe; but in what this conſiſts, I confeſs my utter inability to advance any thing ſatisfactory upon, and ſhall therefore proceed to decide upon a queſtion of ſome importance: Whether a fever can happen in conſequence merely of expoſure to contagion or miaſma; or, Whether a ſtate of debility preſent, however induced, be ſufficient for that purpoſe? In the firſt place, it would appear, from the remote cauſes of fever enumerated by authors, that whatever powers weaken the ſyſtem conſiderably, are the means of producing fever. We find, that exceſs in venery, frequent intoxication, intenſe ſtudy, the application of cold, &c. are conſtantly mentioned as remote cauſes; and if contagion or miaſma were the only powers that occaſion the diſeaſe, it would be very abſurd to mention any other cauſe. But I apprehend, that from actual obſervation it has been concluded, that every debilitating cauſe, if ſufficiently powerful, will itſelf produce a fever; and on this account I preſume it is that different authors have made mention of ſeveral different powers, ſome of which had never been remarked before, and which they found from experience to have given riſe to the diſeaſe. This is ſupported by appealing to the cautions of former phyſicians. Dr Sydenham forbids our adminiſtring purges after a fever, as they endanger the return of it; and Dr Cullen aſſures us, "that when the paroxyſms of a fever have "ceaſed to be repeated, they may be again "renewed, and are moſt commonly renewed, "by the application of debilitating powers*." Dr Grant, who has had pretty extenſive practice in fevers, gives it as his opinion, that whatever deſtroys the tone of the ſolids, and particularly of the firſt paſſages whence ariſe crudities and indigeſtion, are the ſources of fever; and he has therefore reckoned unwholeſome proviſions, and draſtic purges, among the remote cauſes †. And as a further confirmation of this doctrine, how often does it happen that women newly delivered are ſeized with fevers from the loſs of a conſiderable quantity of blood? and yet it cannot be conceived any contagion whatever can have operated in effecting this, though notwithſtanding it is probable that during the diſeaſe a ſource of infection highly dangerous may be generated. Beſides, there are many facts that tend to make this matter almoſt unqueſtionable. Baron Van Swieten obſerves the caſe of a lady who got a quartan in conſequence of a fright; and after ſhe had got well, it returned on account of her being thrown into another fright *. Nay, I have myſelf ſeen three caſes of continued fever, and one of an intermittent, when upon the moſt minute inquiry I was certain theſe patients had not been near any ſource of fever that could be deemed contagious. And when the French took Bergen-op-Zoom, it was thought that the want of good water, and the ſcarcity of freſh proviſions they found there, was the cauſe of the epidemic fever which raged with ſuch conſiderable fury, deſtroying

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* Vide Lind on Fevers and Infection.

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* Cullen's Firſt Lines, Vol. I.

† Grant on Fevers.

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* Van Swieten. Comment.

ing a number of the ſoldiers. From theſe facts, therefore, I conclude, that whatever cauſes weaken the ſyſtem ſufficiently, are to be conſidered as remote cauſes of fever; and that the interpoſition of either contagion or miaſma is by no means abſolutely requiſite for the production of this diſeaſe.

With regard to the operation of contagion and miaſma, it is to be obſerved, that as their origin depends upon the diſſolution of animal and of vegetable matter, ſo there can be little doubt of their proving ſedative when applied to the bodies of men; and the phenomena of fever fully evince this effect to be produced. Doctor Cullen, in ſpeaking of theſe general ſources of fever, ſuppoſes, that contagion is the cauſe of continued fevers, and miaſma produces intermittents. It appears to me, however, his ſuppoſition on this head does not always turn out to be actually the caſe. I apprehend that the circumſtance favouring the operation of contagion, miaſma, or any other debilitating cauſe, is a weakened ſtate of the ſyſtem to which theſe noxious powers are applied. It is more than probable that perſons are in many caſes in ſuch a habit of body as to almoſt reſiſt for a conſiderable time the application of even the depreſſing power of contagion; but admitting it has the effect of diminiſhing the vigour of the ſyſtem in ſome degree, the debility induced may not yet be ſufficient to lay the foundation of a continued fever, without the concurrence of ſome other weakening cauſe. In ſuch inſtances, therefore, it is a probable concluſion, that as the ſyſtem is not injured ſo far as to admit of a continued fever taking place, yet it may be fully enough impaired for the production of a remitting or intermitting fever; and in fact, many fevers beginning as remittents and intermittents, while they have been known to ariſe from miaſma, frequently become continued, which ſhould not happen unleſs the perſons ſo affected had been afterwards expoſed to contagion, but any weakening cauſe will produce the ſame effect. Beſides, Dr Donald Monro mentions, that ſome agues which came under his care, began in the form of continued fevers, which came afterwards to intermit; and in ſpeaking of the remitting fever, he expreſsly obſerves, there were many caſes where the fever went on in a continued form through its whole courſe, without any ſigns of remiſſion, though in every reſpect they had the other ſymptoms marking the diſeaſe; and when the fever came to remit in ſome, it afterwards changed into a continued type*. The yellow fever of the Weſt Indies, which is of the continued form, and defined by Dr Cullen, "Typhus cum flavedine cutis," has often been known to have ariſen from marſh miaſma; which is in direct oppoſition to his idea; and argues, that there the habit of body was ſuch as to favour its operation, and that degree of debility followed as to admit of the diſeaſe taking place. We are informed by Dr Lind too, in his Hot Climates, an hoſpital was built at Jamaica, which was denominated Greenwich for its utility and elegant appearance. Its ſituation, however, was upon a very unhealthy ſpot, and adjoining to a moraſs. The conſequence of this unwholſome ſituation was, that when a patient was ſent thither with a mild intermittent, he was ſeized with a malignant fever, a bloody flux, or ſome other mortal diſtemper. The yellow fever raged very much there, attended with the moſt profuſe evacuations; and it was obſerved, that the recovery of thoſe who had been ill was extremely tedious, while the leaſt indiſcretion ſubjected them to a relapſe †. Upon the whole, then, it appears extremely probable, that the effect of contagion or miaſma in inducing fevers of the continued, remitting, or intermitting type, depends more on the diſpoſition of the body than on any ſpecific power of contagion producing continued fevers, and miaſma intermittents; for if a perſon labours under any other of the remote cauſes of fever, and is at the ſame time expoſed to miaſma, it is as likely he will have a fever of the continued type as an intermittent. We have a proof that contagion does not always induce a continued fever in Dr Ruſſel's work on the Diſeaſes of Aleppo; who obſerves, that the plague ſometimes makes its appearance under the form of an intermitting fever; which argues what I have before urged, that ſome conſtitutions are capable of reſiſting in a great meaſure what in others inſtead of occaſioning an intermittent would infallibly be the cauſe of the moſt dangerous continued fever. And as a further proof that contagion poſſeſſes no ſpecific influence in producing continued fevers, other diſeaſes have followed the application of it; and fevers continuing for ſome time have at laſt turned to the bloody flux, dyſentery, &c. and theſe have left the patient when he has again been attacked with the fever *.

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* Monro's Obſervations, Vol. II.

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† Lind's Hot Climates.

COLD is likewiſe to be conſidered as inducing fever; but its operation upon the body has been ſuppoſed to be ſo diametrically oppoſite in different circumſtances, and which is ſtill involved in ſuch great obſcurity, that I am conſcious any attempt to explain it will be attended with many and perhaps unſurmountable objections: I therefore muſt premiſe, that upon this head I would wiſh to reconcile the oppoſite ideas which

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* Vide Monro's Obſervations, Vol. II. p. *69*. Cleghorn's Minorca Diſeaſes, edit. 3d. p. 134.

which different perſons entertain concerning its operation; and what is here offered, tends to this end.

Dr Cullen, in the laſt edition of his work, has properly divided the power of cold into abſolute and relative. The abſolute power is that by which the temperature of the body is diminiſhed in conſequence of its application; and every degree of cold, or, to ſpeak philoſophically, diminution of heat below 62 degrees of Farenheit's ſcale, is found to lower the temperature of the body, while the degree mentioned operates in the proper medium for the preſervation of health.

The relative power of cold, again, is that by which a ſenſation of cold is produced on account of a greater heat being experienced before the application of a leſs degree of it; and hence it is obvious, that if the oppoſite ſides of the body are expoſed to different temperatures, though in fact both theſe are above 62° of Farenheit, upon this being diminiſhed, a ſenſation of cold will be felt on that ſide where a greater heat has been applied immediately before; though at the ſame time, if the temperature is not lowered more than 62°, this ſenſation of cold will ſoon go off, and the perſon will remain in a proper medium, neither experiencing any conſiderable degree of heat nor cold.

The relative power of cold now ſpoken of, is not to be taken in the limited ſenſe of a ſudden and tranſitory degree, as this does not operate ſo powerfully as to be productive of any bad conſequences; it is only when the application of it continues for ſome time, we are made ſenſible of its morbid effects.

Were we *à priori* to conſider, that as heat is ſo neceſſary for the ſupport of animal life, and which is not immediately evolved even at its ceſſation, it might appear abſurd a diminution of it ſhould occaſion inflammatory diſeaſes; but to explain its operation, it is neceſſary that we conſider thoſe circumſtances which particularly determine it to produce in ſome, perhaps a pleuriſy or rheumatiſm, while again in others a fever of the typhoid ſpecies follows its application. It appears not a little ſurpriſing that phyſicians ſhould not in general have paid more particular attention to thoſe circumſtances which favour or reſiſt the preſence of hurtful powers, as it would in many caſes have afforded a more ſatisfactory explanation of the effect of cauſes than have otherwiſe reſulted from their reſearches. With this view, in the preſent attempt to explain the operation of cold, I muſt obſerve, that whatever difficulty occurs in accounting for it, I primarily have recourſe to the particular prediſpoſition of the body, whereby it happens, that if two perſons of different conſtitutions are expoſed to the operation of cold, they ſhall not both be attacked with the ſame diſeaſe; nay, it is probable, the one may be ſeized with a diarrhœa, while the other gets a pleuriſy. And, ſecondly, if the degree of cold acting, with the manner of its application, is attended to, we may, I conceive, be able to account for its effects.

Cold, then, in its application, may be partial or univerſal; and it is in the former caſe more eſpecially, I imagine, it diſpoſes to inflammatory complaints. In the inducing of theſe, we may obſerve, that the ſurface of the body, as more immediately expoſed to its influence, ſhould be primarily made ſenſible of its effects; and I apprehend this is actually the caſe. We obſerve, upon expoſure to cold, that the ſkin becomes pale, ſhrivelled, and contracted, which thoroughly evinces that the tone of its veſſels is conſiderably impaired. This atony taking place on the ſurface would be the ſource of diſeaſe to the ſkin, provided it was liable to be eaſily affected; but we know for certain, that the outer part of it, the ſcarf-ſkin, poſſeſſes little or no ſenſibility, and when the tone of its veſſels is deſtroyed, their diameters will be leſſened, if not rendered altogether impervious, which will prevent the cutis vera from experiencing ſo forcibly the morbid effects of cold. That the cutis vera does ſometimes feel its effects, I conclude from the pain which ſucceeds the application of cold, when particularly the body has ben more than ordinarily heated; this pain, however, is merely ſuperficial, and is eaſily removed by friction, by the warm bath, and by any other means that will reſtore the tone of the veſſels on the ſurface.

As the phenomena exhibited on the ſurface upon expoſure to cold, plainly demonſtrate that the activity of the veſſels is at this time greatly weakened; hence therefore they are rendered incapable of throwing out the perſpirable matter. The retention of this then will prove an unuſual irritation to the ſyſtem, and in a more particular manner will affect the internal and larger veſſels of the part immediately below that on which the power of cold chiefly exerted itſelf, cauſing local inflammation. Thus, for inſtance, if the breaſt has imprudently been ſubjected to cold, a pleuriſy will more readily occur than any other phlogiſtic diſeaſe; but if the cold applied has been univerſal, a general inflammatory diatheſis will be the conſequence, as in rheumatiſm.

This effect of cold now treated of, I have no doubt is frequently ſalutary as well as morbid. I conceive it to be ſalutary when a conſiderable heat has prevailed for a great length of time before; and which though it cannot be denied that heat is a highly ſtimulant power, yet the diſeaſes of the warm countries are far from being of an inflammatory nature. A general relaxation is the conſtant attendant of exceſſive heat, while in a cold climate a vigour and ſtrength of conſtitution are to be remarked; and tho' cold be conſtrued in the light of a direct ſedative, yet if it be partial in its application, and acts eſpecially on the ſurface by obſtructing perſpiration and determining the fluids inwardly, I can readily conceive an inflammatory diatheſis may be the conſequence: but at the ſame time I am of opinion it requires a prediſpoſition of the body to theſe diſeaſes; and likewiſe that the degree of cold acting be not very much under the temperature of 62° of Farenheit; for provided it is conſiderably below the degree mentioned and long continued, by weakening the vital principle a palſy or mortification may enſue.

The operation of cold as inducing inflammatory diſeaſes is particularly favoured by the effect of ſtimulant powers acting in rather more than the common degree at the time. Thus we are ſenſible, on drinking wine, punch, or any other ſtimulating liquor, of an increaſed diſcharge of urine or of perſpiration; ſometimes the one, ſometimes the other, prevails; but whichever ſecretion is augmented, I apprehend that it tends to obviate any dangerous conſequence which the liquor might have if no increaſed diſcharge followed the uſe of ſuch ſtimuli. Whether, however, this be the caſe or not, it is notwithſtanding ſufficiently certain, that perſons are at this time more liable to be affected by cold; and the conſtant effect of it in ſuch is the production of an inflammatory ſtate of the body, as happens in catarrh, rheumatiſm, and in pneumonic inflammation, &c. At the ſame time, the certainty of perſons being more ſuſceptible of cold when the heat of the body is increaſed by the uſe of inebriating liquors, affords us a proof of the manner in which we have attempted the explanation of it as occaſioning phlogiſtic affections, as it is evident that the pores of the ſkin are then more enlarged to admit of the particles of the perſpirable matter flowing with greater facility; and when it may be imagined the application of cold as contracting theſe, will be more powerful on account of the greater evaporation going on now from the ſurface. Another proof, that the power of cold occaſioning inflammatory diſeaſes operates on the ſurface by obſtructing perſpiration, I apprehend, ariſes from a fact well known, and taken notice of by Dr Huxham and by every epidemical writer, that pleuriſies are moſt frequent in dry cold ſeaſons, and endemic in high cold ſituations.

A ſecond way in which cold (for when I uſe this term, I mean any degree of heat below 62° of Farenheit) may give riſe to inflammations, depends on a conſiderable cold having prevailed for a length of time during the winter, when this has been ſucceeded by a greater heat as in ſpring, ſo that what has been termed cold by authors, and ſaid to have produced inflammatory diſeaſes, may ſtill have operated on the ſyſtem as an increaſed degree of heat. This ſenſation of heat, though perhaps below 62° of Farenheit, depends altogether on the relative power of cold: but whether inflammatory diſeaſes happen generally in conſequence of an increaſed degree of heat, I cannot determinedly ſay; I am inclined to think not. I do not, however, deny the probability of their happening at any time independent of the effect of the air; experience convinces us theſe diſeaſes are to be met with, though they are not very frequent, in every ſeaſon of the year: But ſo far as they are occaſioned by heat ſucceeding cold weather, I would obſerve, that this ſenſation of heat, though the thermometer ſtood below 62° of Farenheit, as depending upon the relative power of cold, might be deemed cold to thoſe who had not experienced the greater cold that preceded the increaſed heat.

Having thus attempted to explain the operation of cold as producing inflammatory complaints, it only remains to account for diſeaſes of debility following its application, and for thoſe circumſtances which eſpecially favour it in this. I have hitherto conſidered cold inducing phlogiſtic diſeaſes, as operating upon the ſurface by deſtroying the vigour of the veſſels; and that in an indirect manner it occaſions theſe affections. But where palſies, gangrene, and other diſeaſes, beſpeaking an impaired ſtate of the vital principle, follow its application, I apprehend, that in theſe caſes the degree of cold has been conſiderable, and acted on a large portion or on the whole body; while at the ſame time a peculiar irritability of ſyſtem has favoured the operation of this power. There are innumerable inſtances, where though cold has been only partially applied, yet in irritable conſtitutions a general indiſpoſition enſues; and when cold is accompanied with moiſture, theſe ailments more readily occur. It has long been remarked, that this ſort of weather gives the prediſpoſition to the ſcurvy, the ſcourge of our fleets and armies; and nothing has been found more effectual in removing this prediſpoſition than warm clothing. In the rainy ſeaſons of the warm climates, weakly people are then particularly liable to be attacked with the diſeaſes of the climate, which they might in a great meaſure prevent by wearing warmer clothes than are commonly uſed in the dry ſeaſons, which is ſeldom obſerved; and the generality of perſons are often ſenſible of a warineſs, yawning, and ſhivering, except thoſe who overcome theſe marks of laſſitude by ſuch exerciſes as can be obtained within doors, who keep fires *, and who live in ſuch a manner the moſt likely to reſiſt the effects of chilly and moiſt weather.

The DEPRESSING PASSIONS come next under conſideration; the effect of them, however, is ſo well known, that I ſhall animadvert but little upon it. They have the moſt directly oppoſite effect to joy: the latter increaſes the circulation; while the former diminiſh the vigour of it, depriving the face of its colour, &c.; and if they are very violent, the powers of life are often ſuſpended for ſome time. In many caſes, they act with ſuch an ungovernable fury as to hurry the

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* Upon an average, in moſt of the Weſt India iſlands, I do not believe above one houſe in twenty have fireplaces; and yet in no part of the world are fires more requiſite than in theſe iſlands in the rainy ſeaſons. Beſides, many houſes, particularly in the country parts, have no cieling; and are only covered by means of pieces of wood, nearly in the ſhape of what is vulgarly called an oblong ſquare, termed ſhingles, and theſe are not united with accuracy, ſo that the rain comes in through the roof; and I have often myſelf got into bed, when the ſheets have been exceſſively damp in conſequence of the rain coming down from the roof.

the patient very ſoon off the ſtage of life, as evidently happens in the ſcurvy. But in every inſtance where the unhappy patient is diſtreſſed by them, they conſtantly add very much to the diſeaſe he labours under *; and it has been repeatedly obſerved, when

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* Dr Lind, in his preſervatives againſt infection, acquaints us, " It is a received opinion that Fear is a " cauſe of itſelf ſufficient to produce, in certain diſpo- " ſitions, a bad or malignant fever. There are at " leaſt many inſtances in beſieged towns, where no " other reaſon could be well aſſigned for the riſe of " malignant diſorders, than the dejection of ſpirits, " grief and panic of the inhabitants, occaſioned by the " bombardment, and the apprehenſions of a violent " death from ſome ſudden aſſault of the enemy. This " much is certain, that ſuch paſſions of the mind ſerve " powerfully to propagate an infection, even the " plague itſelf. So that on all ſuch occaſions, too " much art cannot be uſed to animate with hope and " confidence both the afflicted and the ſound. Spec- " tacles of horror are never to be expoſed to the view " of ſick perſons: thoſe therefore who die ſhould be re- " moved ſilently and privately out of an hoſpital to a " proper place, where no idle ſpectators ſhould be per- " mitted to view the ghaſtly appearance. It is always to " be remembered, that every ceremony that is obſerved " relating to a corpſe, makes a deep impreſſion on " the mind, eſpecially of the afflicted and diſpirited, " and

when a dangerous fever has made its appearance on board a ſhip, that the terror and apprehenſion excited in the breaſts of the people, have occaſioned immediate ſickneſs. Nor need we be ſurpriſed at it, when we reflect how much by the agitation of the mind the body is enervated; the appetite is deſtroyed, the heart palpitates, and ſuch a train of alarming ſymptoms follow, that no wonder a moſt malignant fever ſhould be the conſequence: and I perfectly agree with Dr Fordyce; who conceives, that when the depreſſing paſſions operate upon the ſyſtem, they afford a more reaſonable way of accounting for fevers than all the theory of ſpaſms which have engroſſed ſo much attention †.

Hæmorrhagies, if conſiderable, tend nearly to produce the ſame effects as the depreſſing paſſions. Women in child-bed are often attacked with fevers on account of having ſuſtained the loſs of a great quantity of

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" and by ſuch impreſſions the body is ſurpriſingly affected."

† Vide Fordyce's Inquiry, p. 54.

of blood during their delivery. And I ſhall afterwards make it appear, that by the too liberal uſe of the lancet in fevers many patients have been precipitately haſtened to their graves.

Of thoſe remote cauſes that indirectly weaken the ſyſtem, I have reckoned exceſs in venery, and frequent intoxication. They manifeſtly impair the vigour of the conſtitution, and will act more forcibly when concurring with any other of the remote cauſes of fevers. And I believe it often happens, that wrong cauſes are aſſigned for the production of a fever, ſuch as probably a trifling expoſure to cold, &c. when, upon a more ſtrict inquiry, it might be traced ſome other more powerful cauſe had operated in effecting the diſeaſe.

CHAPTER V.

OF THE DIFFERENCE OF FEVERS, AND THE CAUSES PRODUCTIVE OF THIS.

WITH regard to the difference of fevers in their forms or types, it is to be obſerved, that independent of the inflammatory fever before ſpoken of, and which I have endeavoured to eſtabliſh as really exiſting in ſome inſtances, I have given it as my opinion, all fevers are eſſentially the ſame; and I have now to add, that the cauſes which peculiarly induce one ſpecies of fever before that of another, muſt be ſought for in the ſtate of body to which theſe are applied, or in the degree of power theſe poſſeſs in themſelves. For that the difference obſerved in fevers can only be attributed to theſe circumſtances, we may obſerve, every fever of more than one day's con-

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continuance, depends ſtill on the power of the remote cauſes in inducing frequently repeated paroxſms. And reſpecting the duration of theſe, Dr Cullen has referred it to a diurnal revolution of the ſyſtem, as every paroxyſm of fever is finiſhed within twenty-four hours; and this revolution he has propoſed whether as depending upon the original conformation of the body, or upon the application of powers continually operating upon it, and inducing a habit. But as the Doctor has not favoured us with his opinion in the manner we could wiſh, I ſhall therefore endeavour to demonſtrate, that on the effect of obvious cauſes this revolution depends; and that the phenomena conſtituting it may be made to occur at a more early period than uſual, or be poſtponed till a later time.

It appears to me, that from the earlieſt date of our exiſtence, while the body is not ſubjected to be acted upon by foreign cauſes which afterwards ſenſibly affect us, we are endowed with a vital principle conſtantly liable during life to be manifeſtly influenced by the powers which firſt produced organization of the parts of the body; and to the ſupport of theſe parts, the immediate ſway of the vital principle becomes indiſpenſably requiſite *. What this principle conſiſts in, I apprehend we ſhall never arrive at the knowledge of. Many have conſidered it as inherent in the brain. Poſſibly it may; but I think there can be little doubt of its having exiſtence before the brain or any other part of the body could be demonſtrated. We cannot even ſee how the reception of the male-ſeed into the vagina and uterus ſhould produce the brain of an animal, ſuppoſing its exiſtence neceſſary to the preſence of the vital principle; but in my opinion, though the brain could be demonſtrated, it muſt have owed its being to ſome power, the knowledge of which the wiſe Diſpoſer of all things has put beyond the reach of our capacities for the beſt of purpoſes, as any cer-

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* Dr Gardiner, in his obſervations on the animal œconomy, a work very lately publiſhed, and which I much regret I had not the pleaſure of ſeeing before this treatiſe was finiſhed, alledges that the vital principle muſt be acted on, and he conceives the nerves are the medium through which it is affected.

certainty of it and of future events might prove extremely pernicious and detrimental to our preſervation.

We only know, then, the fact of the continuance of life for a certain time, during which the mind is endowed with a principle influencing in a great meaſure the action of the corporeal part; but how this effect is produced, or how the mind is affected by injuries the body ſuſtains, we are in utter ignorance of. Our deepeſt reflections cannot ſolve it: They only raiſe our aſtoniſhment, while we are bewildered in the reſearch. At the ſame time that we know nothing of the principle which ſupports our exiſtence, yet every day's experience convinces us how much this principle is liable to be actuated by the operation of foreign or external powers. For inſtance, it is neceſſary for the continuation of life, that we eat to nouriſh our frames, without which life would not long continue; and many other circumſtances are requiſite to aſſiſt the operation of that inward power which is implanted in us at the time we ſay an animal is endued with life. Therefore, when, by the effect of foreign cauſes, the activity of the vital principle is diminiſhed, that ſtate, we call Diſeaſe, is the conſequence. And when this alteration from that condition which conſtituted health takes place, it follows that the ſeveral functions of the corporeal part will be impaired in proportion to the injury the vital principle has ſuffered. To apply this more exactly, when the deviation in the due exerciſe of the functions of the body has commenced, this change will become obvious to our ſenſes, that the marks we have of health ceaſe to appear, when a train of other phenomena preſent, depending upon this change. What this again immediately conſiſts in, is another point that remains to be determined; it can only, however, be, in my opinion, an exceſs or defect in the application of thoſe powers which act, though not all at the ſame time, on the vital principle.

FROM what has been advanced, we ſhould be inclined to draw this inference, that powers operating on the living body produce their effect in the firſt place according to the nature of the conſtitution to which they are applied favouring their operation; and, ſecondly, to the degree of energy they poſſeſs. Therefore from theſe circumſtances we look for the event in affecting the diurnal or other revolution of the ſyſtem. For as in a ſtate of health the power of cauſes operates in a ſufficient degree for the continuance of it, ſo we are ſenſible of the alternations of watching and ſleep, of the deſire for food, &c. with other changes to be remarked in the intellectual functions which depend upon the time and degree of power applied producing theſe ſenſible effects; and that the changes depend upon the cauſes already aſſigned is rendered evident from this, that different perſons are ſuſceptible of theſe ſenſations at different times.

It being thus rendered probable that what we term Diurnal Revolution depends upon the operation of external powers, it only remains to inveſtigate this change that happens in the caſe of fever, to explain the reaſon of the repetition of paroxyſms. The occurrence of every paroxyſm of fever muſt be attributed to the effect of the remote cauſes which have operated or are operating upon the ſyſtem; and according to the degree of debility induced, their repetition is therefore to be explained. On this principle it appears, that quartans and tertians conſiſt in a leſſer degree of debility than fevers of a more continued type; and I would not allege with many, that the periods of the acceſſion of the fit are univerſally the ſame, ſo that the paroxyſm of the quotidian commences in the morning, the tertian at noon, and the quartan in the afternoon; for there are many inſtances where the attack begins at a later period than common, and again it may and actually happens at a more early time of the day. But admitting that the periods of the attack are nearly the ſame, yet it ſtill implies what I have before endeavoured to inculcate, to wit, the operation of cauſes upon the body producing diſeaſe; for if, independent of this, there was any thing particular in the diurnal revolution of the ſyſtem affecting the fit of intermittents, remittents, &c. every paroxyſm, whether of quotidian, tertian, or quartan, ſhould invariably come on at one and the ſame period. This, however, is not the caſe; and I am therefore inclined to believe, that according to the operation of the remote cauſes inducing a more or leſs frequent repetition of paroxyſms, their attack begins at this or that time of the day: and in ſupport of this it may be obſerved, that by the adminiſtration of the bark, or any other tonic or ſtimulant remedy, we have it in our power very often to put by the fit for that day; and it is well known, by perſiſting in the uſe of theſe medicines for ſome time, the diſeaſe will be effectually cured. Nay, to render this more clear and evident, I have adduced an inſtance to prove, when ſpeaking of the remote cauſes, that a ſudden and violent agitation of the mind has immediately induced a fever; and numerous caſes have occurred, where perſons being at once expoſed to a very debilitating power, have been ſeized with every ſymptom marking an attack of the diſeaſe. Dr Clark obſerves, that upon viſiting patients labouring under a remitting fever, he has often experienced all the ſymptoms of an attack, though not naturally liable to fevers; and if he had not uſed every method of prevention, it is probable he would have taken the diſeaſe *. From which, I think, there can be little doubt, that the circumſtances affecting the diurnal revolution of the ſyſtem, are owing to powers continually operating; for when a perſon by expoſure to debilitating cauſes gets a fever, he no longer has the deſire for food, and the alternations of watching and ſleep, with other phenomena, ceaſe to return, or if they do, it is not at their wonted periods; which can only, I apprehend, be imputed to the effect the remote cauſes have had; for, provided they are highly debilitating, and more particularly if at the ſame time the conſtitution of the perſon favours their operation, the conſequence almoſt inſtantaneouſly follows their application.

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* Clark on Hot Climates.

CHAP. VI.

OF THE PROGNOSIS IN FEVERS.

FROM the nature of fever, as appearing from the phenomena and the remote cauſes, it is to be attended to, that our prognoſtications, on the immediate attack of the diſeaſe, or at an early period, ſhould be regulated according to the ſeeming malignity of it; and from the conſideration of the cauſe or cauſes known to have induced it; making a proper allowance at the ſame time for the diſpoſition of the body, whether, on reflection, it is more probable his conſtitution reſiſted or favoured the application of them. In continued fevers, a more doubtful prediction ſhould be always formed, than in thoſe fevers which intermit or remit; as when theſe latter prove fatal, it is generally by paſſing into a continued type.

They, however, endanger life, by producing abdominal obſtructions, of which the liver is the moſt common ſeat; and from hence ariſe dropſies, and other diſeaſes of the cachectic claſs, depending on a depraved conſtitution.

As, therefore, fevers are the more dangerous according as they are continued, I ſhall in general point out thoſe ſymptoms which particularly indicate danger; but at the ſame time, in no one diſeaſe to which the human body is ſubjected, do patients oftener recover, than in fevers, when they have been in the moſt hazardous ſituations; and this alteration is often, too, very ſuddenly effected. In ſuch caſes, in the practice I have ſeen, I obſerved, that at the height of the diſeaſe, the ſymptoms, though very alarming, have yet continued for a much longer time than ordinarily, till at laſt the patient has fallen into a ſound ſleep, when a gentle moiſture has prevailed over his whole body; and on waking, to the utter aſtoniſhment of his friends and the byſtanders, when they expected his death, he has felt a ſtrong inclination to eat, and if he is permitted to ſatisfy fy his appetite, it is obſervable he devours his food with the greateſt avidity.

The ſymptoms denoting danger in fevers, I comprehend under the general head of debility ariſing from the real depreſſed ſtate of the living principle; and ſhall follow Dr Cullen in arranging them according as they preſent in the Vital, Animal, and Natural Functions.

The ſymptoms expreſſing a high degree of weakneſs are,

IN THE VITAL FUNCTIONS:

I. IN THE STATE OF THE PULSE, BEING
  - *a.* Quick.
  - *b.* Weak.
  - *c.* Fluttering inſtead of diſtinct pulſations, owing to the impaired action of the heart and vaſcular ſyſtem,
  - *d.* And intermitting.

II. THE TENDENCY TO FAINTING,
  - *a.* Manifeſted when an attempt is made to ſit up, as the blood is not then driven in ſufficient quantity to the brain.

III. IN THE STATE OF RESPIRATION, BEING
  - *a.* Weak.
  - *b.* Frequent.
  - *c.* Short, and when the air is expelled from the lungs as by a ſudden and convulſive motion.

IV. COLDNESS OF THE EXTREME PARTS, WITH A CLAMMY FEEL OF THE SKIN, WHEN LIFE IS SO FAR EXTINCT THAT SUCH A RELAXATION HAS TAKEN PLACE, AS TO RENDER THE VESSELS INCAPABLE OF RETAINING THEIR CONTENTS.

IN THE ANIMAL FUNCTIONS:

I. THE INABILITY AND IRREGULARITY TO PERFORM VOLUNTARY MOTION AS APPEARING IN A

a. Tremulous motion of the hands.
b. The incapacity to thruſt out the tongue.
c. The picking at the bedcloaths, &c.
d. Convulſions.

II. THE IMPAIRED STATE OF SENSATION, AS APPEARING BY

a. Bliſters not raiſing the ſkin.
b. The inſenſibility to light.
c. The loſs of taſte.

III. THE WEAKNESS OF THE INTELLECTUAL FACULTIES; as

a. Loſs of memory.
b. Delirium.
c. Coma.
d. Muttering to himſelf.
e. Want of ſleep.

IN THE NATURAL FUNCTIONS:

I. IN THE STATE OF THE STOMACH; as,

a. Nauſea.

- *b.* Anorexia.
- *c.* Vomiting.
- *d.* Pain of the ſtomach, and ſoreneſs to the external touch.
- *e.* Inflation of the ſtomach.

II. INVOLUNTARY EXCRETIONS; as,

- *a.* Urine.
- *b.* Sweat.
- *c.* Voiding of the excrements.

III. DIFFICULT DEGLUTITION, DEPENDING ON A PARALYTIC STATE OF THE MUSCLES OF THE THROAT.

And in the general ſtate of the whole body, we may reckon, A, The Hippocratic face; B, The eyes half-cloſed, with the eye-balls directed upwards; C, Petechiæ, maculæ, and vibices; D, Hemorrhagies; E, The fætor of the breath; F, Any local affection of the vital parts, as inflammation, ſuppuration, &c. All theſe ſymptoms, or the greater part concurring, will determine our prognoſtic: at the ſame time we ſhould not be too haſty; for from the exhibition of a remedy, a very conſiderable alteration is produced. The preſence of petechiæ is generally conſidered as leading to a bad omen; and yet Dr Lind has obſerved, that a ſingle glaſs of wine has often effected a great change on theſe ſpots: from being black and purple, they have turned red, rough, and ſcaly; and ſuch has been their mutability at times, that a ſudden alteration in the paſſions of the mind has occaſioned their leaving the patient, when a roughneſs has been remarked in their place *.

With the view of aſſiſting us in our determinations of the prognoſis, ſome phyſicians have thought there is ſomething peculiar in fevers, whereby they generally remain a certain time, and terminate on particular days, named therefore Critical. Theſe days are the following; the THIRD, FIFTH, SEVENTH, NINTH, ELEVENTH, FOURTEENTH, SEVENTEENTH, and TWENTIETH, which were founded by Hippocrates: but Dr Cullen is almoſt the only modern author who appears in ſupport of them, and he thinks they apply to the fevers of this country. Notwithſtanding of the extenſive practice this learned phyſician has been engaged in, I yet cannot agree with him in opinion. His reaſons for believing in the doctrine of critical

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* Lind on Preſervatives againſt Infection.

critical days are theſe, and which I ſhall endeavour to render inconcluſive. He firſt remarks, that the animal œconomy is readily ſubjected to periodical movements, both from its own conſtitution, and from habits which are readily produced in it. Secondly, becauſe he obſerves periodical movements to take place in the diſeaſes of the human body with great conſtancy and exactneſs, as in the caſe of intermittent fevers, and many other diſeaſes *.

Without inſiſting much on the inefficacy of theſe arguments, I might at once refer the reader to what has been delivered on the difference of fevers, and of the cauſes producing it, where I have attempted to prove, that the operation of a proper degree of powers is eſſentially neceſſary for the continuance of health; nay, that upon theſe the diſcharge of every function both bodily and mental immediately depend; while I have aſſerted, with marks of the greateſt probability, the difference of fevers is to be attributed to the effect of the remote cauſes; and that this is brought about in one

* Cullen's Firſt Lines, Vol. I.

one of two ways; either they have been violent in their operation, or the conſtitution has been at the ſame time ſuſceptible of ſlight impreſſions; in which caſe I have given it as my opinion, a more frequent repetition of paroxyſms happens, while a leſs frequent recurrence is to be accounted for from the nature of the powers operating being leſs formidable, or when the ſtamina of the patient by no means co-operated with them. Since, therefore, this effect produced in the conſtitution depends on cauſes we can obviouſly trace, what reaſon is there for conceiving that the ſyſtem is ſtill ſubjected to periodical movements, or to habits occaſioned, when theſe can be readily and ſatisfactorily aſſigned to the operation of manifeſt cauſes. Beſides, I would obſerve, it is impoſſible any deviation from a ſtate of health can occur without ſome power having operated in effecting it. This, however, has often eſcaped obſervation; and on this account it has followed, that many phyſicians have imagined the ſyſtem readily undergoes certain changes independent of any ſenſible power: but this is only a mere conjecture ariſing from our ignorance, or from not paying ſufficient attention to the number and force of thoſe cauſes we are affected by. Dr Cullen indeed obſerves, that though the animal œconomy has a tendency to obſerve the critical days, yet it is by the operation of ſome violent cauſe, the terminations happen on the non-critical *. It is evident, therefore, from this ſentence, the doctor muſt on proper reflection conceive, that thoſe powers determining the critical days may occaſion the termination of a fever on a non-critical preceding a critical one, or it may be poſtponed to a non-critical immediately ſucceeding a critical day. In the former caſe, the cauſes have had a greater effect, while in the latter their operation has been probably reſiſted by the conſtitution; and admitting this, which cannot be well denied, it is only an accidental circumſtance that they may operate in a medium between theſe two effects, when the termination of the diſeaſe will be on a critical day. But what can be ſaid in thoſe fevers particularly oc-

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* Cullen's Firſt Lines, Vol. I.

occurring in the warmer countries, where the progreſs of the diſeaſe is ſo rapid, that many patients are cut off even before the firſt of the critical days, as marked by Dr Cullen. There is no periodical movement or any acquired habit which have operated in theſe caſes, but an increaſe of thoſe violently depreſſing powers hurrying on the fatal event. For when that degree of debility is induced in the ſyſtem which is perfectly incompatible with life, death will enſue at that period, whether this be on a critical or non-critical day. Dr Lind, in his Hot Climates, gives us a moſt melancholy picture of the effects of a remitting fever, which occurred to ſome of his Majeſty's ſhips at Batavia. Many were attacked very ſuddenly with a delirium, and were carried off in the firſt fit, but none got the better of the third. The captain of the Panther died though the bark was early adminiſtered on account of its extreme malignity *. And Dr Clark, in treating of the remitting fever, tells us, he was informed by a ſurgeon at Calcutta, that many patients died in the firſt

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* Lind's Hot Climates, p. 98.

firſt fit highly delirious *; which I think admits of ſufficient explanation by ſuppoſing, that at whatever period the ſyſtem is ſo much exhauſted as is not conſiſtent with life, death then will inevitably happen. Nay, I would allege, that by different modes of treatment, the diſeaſe ſhall be prolonged to an unuſual length, or its termination ſhall be effected infinitely more early. If for inſtance a patient is bled once or twice in a fever, I will maintain that patient will be hurried to his grave ſooner than another where this evacuation has not been performed, without any regard to critical days; and again, when tonic or ſtimulant medicines are adminiſtered, and when no evacuations have been employed, I am equally confident the diſeaſe will be put off to a much longer period than in thoſe patients to whom no remedies of this claſs are given †. And it is more than probable

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* Clark on Hot Climates.

† Dr Millar, in ſpeaking of the remitting fever, obſerves, " The duration of the diſeaſe is various. It hath " ſometimes been terminated in death or recovery in a " few days; but it hath alſo been lengthened out for ſeve-

bable owing to this circumſtance Dr Macbride has aſſerted, that when the nervous fever deſtroys the patient, it generally carries him off in the courſe of fourteen days; if on the other hand he can be ſupported to the twentieth day or thereabouts, he for the moſt part recovers †. Nor is it eaſy to imagine, that when the ſyſtem is ſo conſiderably impaired, that the vital principle is no longer ſenſible to the ſtimuli acting upon it; under ſuch a circumſtance, the death of the patient will be poſtponed to the next critical day. Beſides, all the critical days, up to the eleventh, mark the tertian period; but from it the quartan is as regularly obſerved. Dr Cullen himſelf cannot determine what occaſions the periods to be changed about the eleventh day; he, however, takes the fact as certain: but admitting him this, I would aſk, what connection

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" ſeveral weeks, or even for ſome months, with little " variation. But when it is neglected, or improperly " managed, it generally proves fatal at ſome period be- " tween the eleventh and nineteenth day." Vide obſervations an the prevailing diſeaſes of Great Britain, p. 64.

† Macbride's Practice.

the tertian or quartan period has with the continued type in determining the next critical day. For in the quartan there are no leſs than two complete intermediate days between the fits, while in the continued type an exacerbation occurs twice in the twenty-four hours. Not, however, to confine ourſelves ſolely to reaſoning, Sir John Pringle tells us he did not perceive any critical days, nor any certain termination of the diſeaſe (remitting fever), which was longer or ſhorter, according to the manner in which it was treated *; and within theſe four months, a ſlow fever raged very much here. It was conſtantly prolonged to the twenty-eighth day; and in ſome patients I ſaw the termination did not happen till the patient had been ill five weeks, and even longer. Therefore, upon the whole, I am led to conclude, the doctrine of critical days is not to be attended to; though Dr Cullen has endeavoured to ſupport them by facts collected from the writings of Hippocrates, by *Mr De Haen.* And I am thoroughly convinced, that, upon a minute inveſtigation of this matter, were we to note the termination of every caſe of fever we treat, we ſhould find them concluded as often on the non-critical as on the critical days.

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* Vide Pringle on the Diſeaſes of the Army, 7th edition, p. 172.

CHAPTER VII.

OF THE PROXIMATE CAUSE OF FEVER.

MANY theories of the proximate or immediate cauſe of fever have been received; ſome ſtill are retained by thoſe who were taught the doctrine they at preſent adhere to: but I ſhall attempt to point out a few circumſtances leading us to reject moſt of the principal ſyſtems that have engaged the attention of phyſicians: And when we have laid aſide theſe, I ſhall adopt another, being, as I conceive, more ſatisfactory in explaining the ſeveral phenomena of fever; and as having this farther ſupport, of being built on the knowledge and effect of the remote cauſes.

The firſt opinion that I ſhall take notice of is, that of a lentor or viſcidity occurring in the general circulation, which occaſions an obſtruction in the extreme veſſels of the body, and thus gives riſe to the cold ſtage and the other phenomena of fever. But with Dr Cullen I am perfectly convinced, that the exiſtence of ſuch a lentor or viſcidity does by no means appear, or is rendered in any meaſure probable: for, if we reflect, there are not wanting innumerable inſtances of fever, where the attack of the paroxyſm has been ſo very ſudden, that no alteration whatever could have taken place in the fluids of the body, much leſs ſuch a viſcidity that the blood is prevented from entering the extreme veſſels; and hereafter we ſhall be led to conclude, that the cauſes act with greater certainty on the ſenſorium commune.

Another and more univerſal opinion is, that a hurtful matter, received into the ſyſtem, or even produced there, is the proximate cauſe of fever. And further, that the in-

increaſed action of the ſanguiferous ſyſtem is an effort of the vis medicatrix naturæ to throw out this offending matter; and more particularly to produce ſuch a change, as to render it perfectly free of any bad effect; or conſidering it in another light, the making it fit to paſs out of the body by any of the excretories. But tho' this doctrine bears a plauſible appearance, and is of great antiquity, yet it is founded on principles deſtitute of what ought to be our conductors in inveſtigating any ſubject. Beſides, fevers are daily induced by cold, fear, exceſs of venery, &c. with all the circumſtances eſſentially characteriſing this diſeaſe, but at the ſame time without reaſons for preſuming on the preſence of morbific matter: and it can hardly be imagined the cure of febrile diſeaſes depends upon the expulſion of any ſuch matter, or that it would ſatisfactorily account for the phenomena; while Dr Cullen has remarked with great propriety, that the admirers of this ſyſtem have not ſatisfied us with the ſolution how the concoction of the morbific matter is performed, or that any change does in reality take place. And the re-

remedies to be directed for the cure, it is ſufficiently obvious, do not operate upon the fluids. And I ſhall afterwards urge reaſons for rejecting the prevailing notion of a vis medicatrix naturæ, when the expulſion of the morbific matter cannot be aſſigned to this power.

A third doctrine, which has been received, is reſpecting the quantity of bile ejected by vomiting in intermittent fevers; and this occurs ſo very frequently, that many have concluded an unuſual increaſed ſecretion, attended with a peculiar condition of it, gives riſe to intermitting fevers: but this, as well as the preceding opinion, appears entirely void of proper foundation; for in other diſeaſes it is obſerved, that the ſame circumſtance happens where vomiting and purging are peculiar ſymptoms, and more eſpecially when they concur with the influence of warm climates, and in the hotter ſeaſons of this country. A remarkable inſtance of this we have in cholera morbus, a diſeaſe often appearing without a ſtate of pyrexia; but beſides, intermittents, it is generally thought, ariſe from other cauſes more probable, and to obviate the effect of which the medicines adminiſtered give every reaſon to preſume their being of a ſedative nature, as the bark and other ſtimulants are univerſally known to accompliſh the cure; while in a ſubſequent part of this treatiſe it will appear, evacuations, directly operating on the alimentary canal, inſtead of proving beneficial, which they ſhould if a peculiar quality of the bile was the cauſe of theſe fevers, have done conſiderable miſchief, and have induced ſuch a purging, as afterwards could be ſcarcely ſtopped; and hence the redundance of bile ſo often remarked in intermitting fevers, I conclude is a mere ſymptom, connected with and altogether dependent on the ſtate of the ſyſtem. Dr Maclurg conceives, that "whatever accelerates "the ſeptic animal proceſs, has a tendency "to increaſe the biliary ſecretion, ſince it "favours that change of the blood which "fits it for becoming bile. And is it not "in this light we are to view the large ſe-"cretion of bile which ſeems always to be "the conſequence of hot weather, and which "ſhows itſelf more remarkably in the inter-"mittent and remittent fevers ariſing from

" marſh miaſma? For that this ſymptom " is not ſo much the effect of the intermit- " tent paroxyſm, as of a certain ſtate of the " fluids * in hot ſeaſons, appears from an " obſervation of Mr Cleghorn's, that when " the intermittents of the ſummer continued " until the cold weather ſet in, they loſt " their malignity and contagion, and were " no longer attended with the exceſſive re- " dundancy of bile. And the icteric colour " which characteriſes the ſame kind of fe- " vers, is it not to be explained in the ſame " manner? It is remarked by Dr Lind, that " the ſerum of the blood diſtinguiſhed by " this ſymptom, and named on that account " Yellow Fever, had a bitter taſte †."

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* The conſiderable quantity of bile ſecreted in the warm climates, is always an effect, and which accompanies a lax or debilitated habit; for thoſe who are moſt liable to ſtomachic complaints, and are more eaſily affected by trifling cauſes, experience more of theſe diſtreſſing ſymptoms marking a bilious diſpoſition, than thoſe who are of a more vigorous conſtitution. Dr Maclurg has improperly attributed the large ſecretion of bile to the ſtate of the fluids; but the removing of it does by no means tend to correct any particular condition of them. Bark, chalybeates, and elixir of vitriol, are the medicines generally employed in the cure.

† Vide Maclurg on the Bile.

The next and laſt opinion which comes under our conſideration is that of the ſpaſmodic doctrine, which is ſo fully illuſtrated by Dr Cullen. We are told by him, " The
" remote cauſes of fever are certain ſedative
" powers applied to the nervous ſyſtem,
" which diminiſhing the energy of the brain,
" thereby produce a debility in the whole of
" the functions, and more particularly in
" the action of the extreme veſſels; ſuch,
" however, is, at the ſame time, the nature
" of the animal œconomy, that this debility
" proves an indirect ſtimulus to the ſan-
" guiferous ſyſtem, whence by the interven-
" tion of a cold ſtage and ſpaſm connected
" with it, the action of the heart and arteries
" is increaſed, and continues ſo, till it has
" had the effect of reſtoring the energy of
" the brain, of extending this energy to the
" extreme veſſels, of reſtoring therefore their
" action, and thereby eſpecially overcoming
" the ſpaſm affecting them; upon the re-
" moving of which, the excretion of ſweat,
" and other marks of the relaxation of ex-
" cretories, take place*." This is the outline

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* Vide Cullen's Firſt Lines, Vol. I.

line of the ſpaſmodic doctrine; and tho' it bears marks of conſiderable ingenuity, yet I apprehend we are by no means to adopt it. For, in the firſt place, Dr Cullen acknowledges that debility lays the foundation of fever, and that he can more readily perceive how debility induces ſpaſm than ſpaſm debility; and from hence he draws a very juſt inference, "that the degree of ſpaſm formed, "and the obſtinacy of its continuance, depend in many caſes upon the power of "the cauſes inducing debility, and upon the "debility induced; for the more powerful "the debilitating cauſes, and the greater the "debility produced, the paroxſyms are the "longer, and the more frequently repeated*." This I conceive to be perfectly juſt, and cannot but regret, after his having mention this much, he ſhould have fallen into farther reaſoning. It is perfectly ſufficient to elucidate by far the greater part of the phenomena that attend fever; and tho' I am thoroughly perſuaded that the whole chain of ſymptoms depend upon debility, yet I allow, ſome do not admit of ſo eaſy an explanation, and it is to be feared we may, reſpecting ſuch, for ever remain in the dark.

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* Vide Firſt Lines, Vol. I.—It has been objected to this, that the quartan is the moſt difficult of cure of the intermitting types; and were the above account juſt, it ſhould admit of the moſt ready cure, becauſe the repetition of the paroxſyms is not ſo frequent. That the quartan does not ſo ſoon yield to remedies as the other types, I can readily allow: but ſtill I maintain, that the debility is leſs than what it is in other intermittents; for we obſerve, that in every diſeaſe, according to the degree of debility, it ſooner or later terminates; the greater

Dr Cullen further tells us, that the ſpaſm is the effect of the vis medicatrix naturæ to relieve the debility; but what proof is there brought for the aſſertion that we poſſeſs ſuch a power? I apprehend there is none; for

greater the debility, the ſooner a fatal or happy event. Nor is it eaſy to conceive, that when the ſyſtem is extremely injured, it is then capable of withſtanding ſuch a ſhock for a conſiderable time; the iſſue muſt be determined within a ſhort period. The quartan therefore, as a chronic diſeaſe, conſiſts in a particular ſtate of debility not in itſelf great but at the ſame time requiring the conſtant exhibition of tonic remedies to overcome it; for the fact is undoubtedly certain, chronic diſeaſes admit not ſo eaſily of cure as acute ones, though at the ſame time no one will allege that the debility in chronic affections is ſo great as in acute diſeaſes.

for the Doctor appears to have drawn this concluſion from merely conſidering the phenomena, that when the cold ſtage has continued for ſome time, it is ſucceeded by a hot fit. Why the hot ſhould generally ſucceed the cold ſtage, I can by no means ſatisfactorily explain. We are therefore, then, under the neceſſity of taking the fact; and notwithſtanding Dr Cullen's firſt indication in the cure, the moderating the violence of reaction, yet I have frequently ſeen ſtimulating remedies remove all the ſymptoms attendant on the hot ſtage, the anxiety, thirſt, the throbbing and pain of the temples, &c. and have been the means of procuring ſleep, out of which the patient has awoke in a great meaſure free of fever, and which could not be ſuppoſed to happen if the reaction in any meaſure required the uſe of evacuants. Dr Cullen indeed ſays, that if ever ſtimulating remedies have been of uſe, they prove ſerviceable by their anti-ſpaſmodic power. I ſhall ſpeak more fully hereafter on theſe remedies; and from their effect I ſhall be led to conclude they operate by removing the ſymptoms of debility, and that this is accompliſhed by directly ſtimulating. But further, is it conſiſtent in any ſhape, that if we allow the ſpaſm and the conſequent reaction to be an effort of nature to relieve the debility, why at the ſame time moderate that very power which is operating towards the removal of the diſeaſe? Does it not on the contrary appear far more conſiſtent, that we ſhould uſe our influence in aſſiſting, rather than checking, nature in her effort. I apprehend it does; and if we conſult the writings, particularly of thoſe authors who have practiſed abroad, we ſhall be deterred from following any method of cure that tends to debilitate the ſyſtem. Dr Clark tells us, that in three patients whom he bled at Culpee, the conſequence was, "the firſt did not bear the evacuation, his "pulſe flagged, and he was very delirious in "the enſuing fit, the remiſſions were very in-"ſenſible, and the exacerbations were only "to be known by his delirium. The others "were ſeized very ſuddenly, and fell down "in a deliquium: on opening a vein, they "returned to their ſenſes; but before five "or ſix ounces of blood were taken, they

" became faint, and the feveriſh paroxſyms " ran higher than in thoſe who did not " ſuffer the evacuation *." Dr Lind too, in his diſſertation on the marſh Bengal fever, mentions one of his patients who was not only extremely deſirous to be bled himſelf, but he excited in the others the ſame deſire. The Doctor therefore, to get rid of their importunities, let blood from him who had firſt requeſted it; the conſequence was, that he immediately loſt his ſtrength, and within an hour, during which time he made his teſtament, he died in the next fit †. The further dangerous effects of blood-letting are exemplified by Dr Badenoch in the fourth volume of the Medical Obſervations. He informs us, that in the fevers of the Eaſt Indies, this operation is far from being adviſeable; on the contrary, it is extremely prejudicial, notwithſtanding the fever be high. He bled two patients, one of whom died the day following, the other fortunately eſcaped by taking the bark. And during the rage of the Joanna fever, he always expected to procure an intermiſſion, or at leaſt re-

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* Clark on Hot Climates, p. 136.

† Lind's Theſis, Engliſh copy, p. 53.

remiſſion, by the uſe of evacuations: but he was much diſappointed in this; for the fever aſſumed a continued type, and now and then it had violent exacerbations, under which ſeveral were carried off. And being apprehenſive he would loſe the greater part of thoſe that were then ill, he without loſs of time adminiſtered to between thirty and forty patients labouring under the various ſtages of that fever, one drachm of the Peruvian bark with ſome wine every hour. Several of them were at this time ſeemingly within a few hours of their diſſolution, with the pulſe ſunk, and an almoſt univerſal coldneſs of the whole body; who notwithſtanding, after they had taken a few doſes of the bark, turned conſiderably better, and ſpeedily recovered. And he obſerved, that this medicine, inſtead of retarding the natural ſecretions, on the contrary promoted their diſcharge, more eſpecially if the patients had not ſuffered any evacuations previous to its exhibition; and that thoſe who got the bark the earlieſt, recovered more perfectly than others, who by the effects of evacuants, together with the ſeverity of the diſeaſe, had been much exhauſted before it was given *.

Dr Sandiford too, ſpeaking of an epidemical diſtemper that was prevalent in the iſland of Barbadoes, acquaints us, in the hot ſtage of this diſeaſe, (which he denominated a Putrid Remitting Fever) the pulſe was ſo ſtrong, quick, and full, as to have led many to order blooding: yet ſuch was the effect of this procedure, that the patients ſo treated were reduced to the moſt extreme pitch of weakneſs; and he mentions a caſe where this evacuation had been repeated in a ſtrong robuſt man, that he was ſunk ſo remarkably low in conſequence of it as to drink ſeveral pints of Madeira wine before he was reſtored to any degree of ſtrength or ſpirits. The like nearly fatal event, he obſerves, has been known to ſucceed the adminiſtration of one or two doſes of nitre, and even a ſmall doſe of rhubarb has induced ſuch a purging as could with difficulty be afterwards mitigated or reſtrained *.

Other

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* Medical Obſervations, Vol. IV. p. 162 and 166.

† Ibid, p. 308.

Other evacuations, as well as the more debilitating one of blood-letting, have likewiſe been attended with the worſt effects. Dr Lind informs us, "That large doſes of "antimonials, or even ſmaller ones too fre"quently repeated, have ſometimes brought "on evacuations, which have entirely ſunk "the patient *." And we are aſſured by Dr Donald Monro, whoſe extenſive practice undoubtedly led him to the obſervation, that James's powder has frequently done conſiderable hurt; for he has known ſeveral inſtances where it has been adminiſtered in putrid ulcerated ſore throats, and in low fevers, and occaſioned ſuch a purging as to haſten the patients to their graves. Neither did Dr James himſelf think that his medicine was endowed with any ſpecific influence in the cure of fevers; but as far as he conceived a fever admitted of cure, it was to be accompliſhed by the Peruvian bark: this, we are informed by Dr Monro, was what Dr James repeatedly profeſſed to him †.

As, therefore, ſuch dangerous conſequen-

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* Lind on Hot Climates, p. 261.

† Monro's Obſervations, Vol. II. p. 13 and 15.

ces ariſe from evacuations in the warmer countries, I contend that the ſame ſedative power, tho' in a leſs degree, will take place in the more temperate regions *; and on this account I cannot ſufficiently condemn the evacuating plan which has been ſo generally employed, as tending to increaſe the cauſes of the diſeaſe, by debilitating the ſyſtem already labouring under a ſtate of extreme weakneſs. And even in this climate, it appears from the two firſt caſes related by Dr Millar in his work on the Prevailing Diſeaſes of Great Britain, that the lancet had deſtroyed thoſe patients; but when he altered his plan, ſubſtituting the bark for blood-letting, the event was the moſt happy: and in the third caſe, when it was certain the repeated bloodings had been of the greateſt detriment, yet by adminiſtering the bark before that period came on, when probably

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* Dr Huxham has known a doſe of phyſic injudiciouſly given in the beginning of a ſlow nervous fever inſtantly followed by exceſſive languor, ſyncope, and other bad ſymptoms; and he obſerves, that though it may be requiſite at the beginning ſometimes to order a vomit or purge, yet every thing draſtic aggravates the diſeaſe. Huxham, Obſerv. Vol. I. p. 48.

bably every medicine would have failed, the diſeaſe was effectually cured, and the patient reſtored to perfect health *. Nor need we be ſurpriſed at the pernicious conſequences ariſing from a number of evacuations when the ſtrength of the ſyſtem is much impaired, if we reflect how eſſential it is that it be excited in a proper degree for the preſervation of health. If previous to theſe evacuations, and particularly to the highly debilitating one of blood-letting, it was duly conſidered how liable we are to be affected by the application of weakening cauſes, and at the ſame time how neceſſary it is that a conſtant ſupply of aliment be received into the conſtitution to ſupply the abraded parts, &c. and particularly when we daily obſerve thoſe who eat and drink moderately, and make uſe of other powers to invigorate the ſyſtem, prepare no more blood than is conſiſtent with health, while the violent reaction, the conſequence of a vis medicatrix ſo much contended for, does not in reality exiſt; we muſt be led to conclude how prejudicial theſe ſeveral ways of

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* Vide Millar's Diſeaſes, p. 91.

debilitating the ſyſtem muſt prove, though they were employed in health; then what excuſe can be found for thoſe phyſicians who have recourſe to them when the vigour of the ſyſtem is conſiderably impaired by the conjoined effects of ſeveral exhauſting and weakening cauſes? A certain determined quantity of blood in the ſyſtem is neceſſary to give the veſſels that proper degree of diſtention for the due performance of their office; and when this comes to be leſſened, a ſhrinking, paleneſs, and diminiſhed force of coheſion between the fibres of the body, are the never-failing reſult. And I would remark, that though there are many diſeaſes which require the uſe of the lancet, yet it is too often employed, and the quantity of blood drawn on many occaſions far exceeds what ought to have been taken. And at the ſame time, I am very confident, there are diſeaſes, where the phlogiſtic diatheſis is not great, and which would be as effectually removed by the adminiſtration of purges, and other medicines of that nature, as by the common and more depreſſing remedy of blood-letting.

In further oppoſition to the vis medicatrix naturæ, I would advance, that during the application of debilitating cauſes, and before that degree of debility is induced in the ſyſtem which conſtitutes a fever, this power ſhould be excited into action, either to prevent the diſeaſe upon expoſure to hurtful cauſes, or to obviate any dangerous effect which the debility already produced might have: but this is not found to be the caſe; and during the cold fit, when the vis medicatrix is imagined to be operating in forming the ſpaſm, it is at this period that we loſe our patients. There are alſo many ſymptoms that take place in the courſe of the diſeaſe, which, upon the preſumption of ſpaſm, appears to me inexplicable; and how comes it too, in many caſes, the diſeaſe continues with unabated violence, when we have an undoubted evidence of this ſuppoſed ſpaſm being overcome? Dr Lettſom makes mention of a caſe * where the patient ſweated, but without any relief, which could not happen if a ſpaſmodic conſtriction

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* Lettſom's Memoirs, p. 94. See alſo Gregory's Elements, p. 47.

ſtriction exiſted on the ſurface. It may be, however, objected here, that this was a caſe of putrid fever, as well as the others recorded by Dr Lettſom: but I muſt beg leave to diſſent from this; as the ſweating continued for ſome time, before any ſymptoms which could be deemed putreſcent enſued. We read likewiſe, in Dr Clark's work on fevers, of ſeveral caſes where the ſweating continued for a conſiderable time, but without any mitigation of the fever; and the patients did not recover until the bark and other tonic medicines were adminiſtered in conſiderable doſes. And as a further proof that Dr Cullen's notion of the vis medicatrix naturæ, and of the cold ſtage, is utterly void of proper ſupport, Mr Cleghorn remarked, that the moſt ſevere paroxyſms of fever which occurred to him, were thoſe that commenced with a burning heat, without any previous cold fit*. This alone is ſufficient to overthrow Dr Cullen's idea. He conſiders the hot as the immediate conſequence of the cold fit; that the vis medicatrix naturæ, during the cold, induces a ſpaſm in the extreme veſſels of the body, which ſupports the hot ſtage: but in the inſtances mentioned by Mr Cleghorn, according to Dr Cullen's own account, it would even appear the vis medicatrix was not excited in inducing any ſpaſm, for there was no cold fit; and yet the paroxyſms were more violent than if the attack of the diſeaſe had been uſhered in with the ſhivering, as is moſt generally the caſe. Dr Cullen would ſtill inſiſt, however, there is ſome degree of ſhivering conſtantly to be remarked; and that in the caſes alluded to, this, on account of its being ſlight, had eſcaped Mr Cleghorn's obſervation. But the late Dr Gregory has likewiſe found the cold fit wanting; and Sir John Pringle, in ſpeaking of the autumnal remitting fever, particularly mentions that the paroxyſm returns in the cloſe of the day, but without any cold ſtage. If, therefore, there was any cold fit to be remarked, it is merely impoſſible it could have paſſed unnoticed by theſe gentlemen, who have been ſo attentive in noting the phenomena and cauſes of diſeaſe.

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* Cleghorn on the Diſeaſes of Minorca.

Dr Cullen further mentions, that with the ſpaſm an atony ſubſiſts in the extreme veſſels: and in ſpeaking of the difference of fevers, we are informed, that the protraction of paroxyſms depends upon two circumſtances; the one is the preſence of a phlogiſtic diatheſis, the other he attributes to a weaker reaction. But Dr Cullen ſurely cannot conceive, that in typhus, though a continued fever, there are marks of a phlogiſtic diatheſis; for in direct oppoſition to what he has delivered in one part of his work, he tells us here, " That all the cauſes of the " diatheſis phlogiſtica have a tendency to " change intermittent into continued fe" vers," when we were before told, that the duration and more frequent repetition of paroxyſms, depend on the power of the weakening cauſes, and to the degree of weakneſs induced in the ſyſtem. Beſides, the making ſynocha and typhus depend on the ſame proximate cauſe, to wit, ſpaſm, implies an inconſiſtency that cannot be admitted. I therefore am of opinion, that in all the ſpecies of intermitting and remitting fevers, and in typhus, a very oppoſite proximate cauſe induces them than what produces ſynocha;

nocha; and wherever a more frequent repetition of paroxyſms takes place, it is entirely owing to the preſence of a greater ſtate of debility. And this receives additional ſtrength, if we conſider what Dr Cullen has himſelf alleged, that when fevers change their type, it is in the following manner: Both tertians and quartans change into quotidians, quotidians into remittents, and theſe laſt become of the moſt continued kind *. And in further confirmation of this, in thoſe diſeaſes which are truly ſpaſmodic, as the emproſthotonos and opiſthotonos, &c. that every remedy which can poſſibly give riſe to a phlogiſtic diatheſis, inſtead of retarding, we know for certain proves the cure.

And laſtly, to conclude my obſervations upon this doctrine, I would remark a ſtrange deficiency reſpecting it. We are informed by Dr Cullen, that the paleneſs and ſhrinking of the external parts of the body, together with the ſuppreſſion of the ſeveral excretions, may be imputed (and ſurely very juſtly) to the weaker action of the heart, in not

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* Cullen's Firſt Lines, Vol. I.

not being capable to propel the blood into the extreme veſſels; yet as they frequently remain after the action of the heart is reſtored, he therefore preſumes a ſpaſmodic affection has taken place. Now the queſtion, that would here ſtrike even the leaſt attentive obſerver, is, when the action of the heart comes to be reſtored, that as this will have the effect of giving the proper tone to the extreme veſſels, and therefore, even according to Dr Cullen's own hypotheſis, of relaxing the ſpaſm, what cauſe is then to be aſſigned for the continuance of the diſeaſe? He conceives an atony of the extreme veſſels is a principal part of the proximate cauſe of fever, and that the relaxation of the ſpaſm depends upon the reſtoring the tone of theſe veſſels; ſo it will be obvious, when the action of the heart is re-eſtabliſhed in its due degree, as this will remove the loſs of tone on the ſurface, no atony, and of courſe no ſpaſm can exiſt, when the continuance of the diſeaſe cannot be attributed to either of thoſe powers which he has aſſigned as making together the proximate cauſe of fever.

ver *. Beſides, I would further obſerve, Dr Cullen's idea of ſpaſm appears in itſelf extremely vague. It certainly implies, that the rigidity on the ſurface is ſo conſiderable, and attended with ſuch energy and power, as to reſiſt the impulſe of the fluids into the extreme veſſels, while they at the ſame time are deprived of their proper degree of activity. The Doctor ſeems to be ſenſible of this objection to his ſyſtem; but as the explanation of it without ſuch reſource would be extremely difficult, he takes it for granted as a matter of fact, and proceeds accordingly. The abſurdity of this, however, appears at firſt ſight: it is to ſay, that a perſon ſhall at once be affected with an idiopathic inflammatory diſeaſe; while his functions, nay even thoſe functions, the proper exerciſe of which are immediately requiſite for the enjoyment of health, ſhall be conſiderably weakened and impaired.

I come now to deliver my opinion concerning

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* I cannot ſee with what propriety any two ſuch powers, as ſpaſm and atony, can be conceived as conſtituting the proximate cauſe of a diſeaſe.

cerning the proximate cauſe of fever, and hope to do it with great probability. It is univerſally allowed, that the knowledge of the remote cauſes of diſeaſe is eſſential to our entertaining a probable idea of the proximate or immediate cauſe. Dr Gaubius very juſtly remarks, "That "the proximate cauſe alone conſtitutes "the whole diſeaſe, and is inſeparable "from it: it is for this reaſon it is call-"ed continent, as it contains the whole "ſum of the origin of the diſeaſe, which "conſtantly manifeſts itſelf when it is "preſent; and it being taken away, the "diſeaſe is likewiſe taken away. The re-"mote is only a part of this, whatever part it "be; ſo only produces a part of the diſeaſe. "Whoever knows all and every one of the "remote cauſes, as they conſpire together, "has a proper idea of the proximate; and "he who takes away all of them, deſtroys "the proximate, and with it the diſeaſe *. From

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* Gaubius's Pathology, Engliſh Tranſlat. p. 17. Dr Gaubius ſhould have ſaid, that if all the remote cauſes are removed, *provided they have not ſufficiently produced their*

From this I aver, that the proximate cauſe depends entirely on the effect of the remote; and this to be univerſally the caſe in every diſeaſe afflicting the human body. And at the ſame time, conceiving this as a general rule without in fact any exception whatever, I am much at a loſs to underſtand how the effect of the remote cauſes ſhould not always have been adopted as the proximate. Some phyſicians, merely ſpeculating on the train of ſymptoms attendant on a diſeaſe, have often conſidered that moſt of the phenomena were in conſequence of ſome particular one, adopting that indeed which at the ſame time would admit of the moſt ſatisfactory explanation of the other phenomena as depending upon it; and this, though only in reality a mere ſymptom, they have taken for granted as the proximate cauſe of the diſeaſe. This has been more particularly the caſe with reſpect to the ſpaſmodic doctrine: for if any ſuch ſpaſm does exiſt, it is altogether

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their effect, the proximate is deſtroyed together with the diſeaſe. For even before the attack of a diſeaſe, all the remote cauſes ſhall have ceaſed from acting, and yet the diſeaſe in very many caſes follows.

gether connected with and dependent on the ſtate of the ſyſtem; and hence can only be conſidered in the light of a mere ſymptom, and by no means as conſtituting the proximate cauſe of fever, any more than the heat, thirſt, or anorexia, which are obſerved to take place. From this it is plain, I build my doctrine of the proximate cauſe of fever on the knowledge of the remote cauſes; and as theſe undoubtedly operate in producing a ſtate of debility, I define the proximate cauſe to be, That condition of the body induced by the operation of the remote. And in proof of this, I have before alledged that ſtimulating remedies have frequently removed all the ſymptoms of fever. But before mentioning the facts to corroborate this, we are firſt led to inquire into thoſe circumſtances which have been hitherto the means of rejecting the uſe of theſe medicines. Dr Cullen ſays, as before-mentioned, if ſtimulating remedies are of ſervice, that they operate as antiſpaſmodics: But admitting they prove beneficial by their antiſpaſmodic effect, why ſhould not he conſtantly employ them as ſuch? He tells us, indeed, "that

" opium, camphor, and muſk, and perhaps
" ſome others, have been employed in fevers
" with advantage; but the circumſtances
" in which they are eſpecially proper and
" ſafe, he finds it difficult to aſcertain, and
" therefore cannot venture to lay down any
" general doctrine concerning them *." It is evident, therefore, from this, that the Doctor conceives they may be ſometimes proper; but the circumſtances in which they are more particularly ſo, he is at a loſs to determine. But I apprehend, from an inveſtigation of his own ſyſtem, we ſhould not have left the matter ſo undetermined as he has done, whether theſe medicines ſhould be adminiſtered or not, or at leaſt we would under certain ſituations have allowed or condemned their uſe. He tells us, " As
" the hot ſtage of fever is ſo conſtantly
" preceded by a cold ſtage, we preſume that
" the latter is the cauſe of the former; and
" therefore, that the cauſe of the cold ſtage
" is the cauſe of all that follows in the courſe
" of the paroxyſm." " To diſcover the
" cauſe of the cold ſtage of fevers, we may
" ob-

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* Cullen's Firſt Lines, Vol. I.

“ obſerve, that it is always preceded by
“ ſtrong marks of a general debility pre-
“ vailing in the ſyſtem. The ſmallneſs and
“ weakneſs of the pulſe, the paleneſs and
“ coldneſs of the extreme parts, with the
“ ſhrinking of the whole body, ſufficiently
“ ſhow, that the action of the heart and
“ larger arteries is, for the time, extremely
“ weakened. At the ſame time the languor,
“ inactivity, and debility of the animal mo-
“ tions, the imperfect ſenſations, the feel-
“ ing of cold while the body is truly warm,
“ and ſome other ſymptoms, all ſhow, that
“ the energy of the brain itſelf is on this
“ occaſion greatly weakened; and we pre-
“ ſume, that as the weakneſs of the action
“ of the heart can hardly be imputed to any
“ other cauſe, this weakneſs alſo is a proof
“ of the diminiſhed energy of tne brain.”
“ We ſhall hereafter endeavour to ſhow, that
“ the moſt noted of the remote cauſes of
“ fever, as contagion, miaſma, cold, and
“ fear, are of a ſedative nature; and there-
“ fore render it probable that a debility is
“ induced *.” From a review of the doc-

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† Cullen's Firſt Lines, Vol. I.

trine here delivered, it will at once ſtrike us, that we ſhould have expected the cure to be obtained from the exhibition of tonic and ſtimulating remedies: for we are told, that the cold ſtage is to be imputed to a debility produced in the ſyſtem, and that all the ſucceeding ſymptoms in the courſe of the paroxyſm depend upon it; whence the concluſion is obvious, the diſeaſe conſiſts altogether in a weakened ſtate of the ſyſtem. And ſince this is the caſe, how unpardonable does it appear in Dr Cullen to have left the uſe of tonics and ſtimulants in ſo ambiguous a manner, as from the foregoing account they immediately ſtrike at the cauſes and root of the diſeaſe? And I will venture to affirm, that in every caſe of fever, where the remote are debilitating cauſes, theſe medicines ſhould be conſtantly employed; nay, in fact, I have repeatedly ſeen the adminiſtering of them attended with the moſt happy effects, which there is no reaſon to allege are owing to any antiſpaſmodic power independent of their ſtimulant; and moreover, I am perfectly convinced their antiſpaſmodic effect is ſolely to be imputed to the ſtimulant power they poſſeſs; as Dr Cullen himſelf affirms, that the ſpaſm depends upon the application of ſedative cauſes, and that the degree of it formed is in proportion to the debility induced.

Many phyſicians reject the uſe of ſtimulating remedies during the hot fit, which they employed during the cold ſtage and in the intermiſſion; but I apprehend, no good reaſon can be aſſigned for deſiſting from the uſe of theſe medicines. They univerſally allow, that the cold fit is owing to debility; for, in proportion to the degree of debility, its violence and duration is conſidered as conſtituting the very principle on which the cold fit depends. On the commencement of the hot ſtage, they, however, adopt a very different notion; and conceive now, that the ſyſtem, inſtead of requiring tonics and ſtimulants, on the other hand will allow every mode of evacuation; and the antimonial preparations with other like remedies they make uſe of, convince us ſufficiently of their putting in practice what they entertain in theory. But I am fully perſuaded, that the hot fit depends on the ſame cauſe as the cold; and that the ſame remedies do in reality remove them both. Without regard to this, however, does it appear any way probable, that the ſyſtem ſhould be ſo much changed in ſo ſhort a courſe of time as takes place from the end of the cold till the beginning of the hot fit, that from labouring under extreme weakneſs, it ſhould acquire ſuch an invigorated ſtate as to admit of evacuation? I am of opinion, there is not the ſmalleſt foundation for this idea; and in conducting our practice, we are not to be led away by any ſuch view; for provided we pay ſufficient attention to the ſymptoms that take place, we ſhall conclude with greater certainty, that this, as well as every other period of the diſeaſe, depends upon debility. It may then be aſked me, what occaſions the great difference between the cold and hot fit; as it is obſerved, that the pulſe during the cold is quick and feeble, while in the hot ſtage it turns fuller and ſtronger. To this I before confeſſed my ignorance, and was under the neceſſity of taking the fact; but as a proof of what I then alleged, Dr Lind informs us of his "having twenty-five patients labour-"ing under intermitting fevers; he pre-"ſcribed an opiate for each of them, to be "taken immediately after the hot fit, pro-"vided the patient had then any inquietude, "head-ach, or ſimilar ſymptom uſually "ſubſequent to the fever. The conſe-"quence was, that nineteen in twenty-two "received immediate relief; the other three "had no occaſion to take it. Encouraged "by this ſurpriſing ſucceſs, he next day "ordered the opiate to be given during the "hot fit. In eleven patients out of twelve, "to whom it was thus adminiſtered, it re-"moved the head-ach, abated the fever, "and produced a profuſe ſweat; which "was ſoon followed by a perfect intermiſ-"ſion *." And I have frequently ſeen the ſame happy event from the exhibition of wine, hartſhorn, and other ſtimulating medicines, in continued fever; the operation of which I conceive to be perfectly analogous to that of opium. It may not be foreign to my ſubject to inſert what Dr Lind has found from experience to be the effect of opium.

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* Lind on Hot Climates, p. 342.

opium when given in the hot fit, as it will at once impreſs a very forcible idea of the propriety of its exhibition. The *firſt* effect is, "it ſhortens and abates the fit; and this "with more certainty than an ounce of "bark is found to remove the diſeaſe. 2*dly*, "It generally gives a ſenſible relief to the "head, takes off the burning heat of the "fever, and occaſions a profuſe ſweat: This "ſweat is attended with an agreeable ſoftneſs of the ſkin, inſtead of the diſagreeable burning ſenſation which affects "patients ſweating in the hot fit, and is always much more copious than in thoſe "who are not under the influence of opium. "3*dly*, It often produces a ſoft and refreſhing ſleep to a patient tortured in the agonies of the fever, from which he awakes "bathed in univerſal ſweat, and in a great "meaſure free from all complaints *." As theſe ſeveral effects of opium may appear ſingular in the eyes of many, and particularly the diminution of the heat of the body with the ſweating; I would therefore offer my ſentiments to explain why it is pro-

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* Lind on Hot Climates, p. 343.

duced. In every diſeaſe affecting the human body, the cauſes I am of opinion operate either in increaſing the tone of the ſyſtem, or on the other hand in deſtroying it. The effect of theſe circumſtances we are generally made ſenſible of by the pulſe. In the caſe of typhus, an increaſed frequency of pulſations takes place, while the momentum at the ſame time is conſiderably impaired; and it is generally allowed, that the ſtrength of the ſyſtem depends infinitely more on the momentum than on the frequency of the pulſe. So I would attempt the explanation of the effect of opium in diminiſhing the heat of the body with the profuſe ſweats that ſucceed, in the following manner. In febrile diſeaſes depending upon debility, it is univerſally known, a conſiderable degree of heat takes place; and this heat of the body riſes above that degree which the very ingenious Dr Alexander hath found requiſite to induce ſweat: and as therefore this effect can only, with any certainty, be attributed to the increaſed number of pulſations in the arterial ſyſtem, and to the diminiſhed momentum; hence it follows, that thoſe remedies which ſuppreſs the increaſed frequency and add to the momentum of the pulſe, are the proper remedies to be employed. In the caſe of inflammation, where both the momentum and number of beatings are increaſed beyond the due degree, the cure conſiſts in diminiſhing both the one and the other; and the remedies to be employed muſt be thoſe which by their ſedative effect are adapted to remove the inflammation exiſting. It is in this way then, that Dr Lind found opium, and Dr Lettſom bark, to be medicines very powerful in exciting ſweat, when given in the hot fit of theſe fevers, by increaſing the momentum and diminiſhing the frequency of the pulſe. As therefore the heat of the body was above the degree requiſite for ſweating, at the time Dr Lind adminiſtered the doſe of opium to his patients, owing to the increaſed pulſations of the vaſcular ſyſtem; hence it follows, that when theſe are diminiſhed, and momentum added to the circulation, then ſweat makes its appearance. The other effects of opium, of ſhortening the fit, and of relieving the violent headach, together with procuring ſleep, admit of ſolution by its obviating the cauſes of the fever, when the body is ſo much reſtored to a healthy ſtate, that theſe ſymptoms denoting great debility no longer continue.

Beſides the prejudices which have prevailed to the giving ſtimulants during the hot fit, many phyſicians have even delayed the exhibiting of them till a perfect remiſſion or intermiſſion has taken place. That this delay is void of foundation, will appear ſufficiently obvious from what has been already delivered; and the opinions and facts of ſome authors, who have ſeen the neceſſity of the immediate application of theſe remedies, will in a great meaſure put this beyond the poſſibility of a doubt. Dr Lettſom tells us, " That after the evacuations he deſigns have been produced, he commences " the exhibition of the bark, without waiting for remiſſions and intermiſſions; a " ſedulous attention to which, and to criſes " in fevers, he preſumes has deſtroyed more " than famine and Sydenham's cold. A dry " dark-coloured tongue, a dry ſkin, urine " without ſediment, deſipientia, delirium,

" dyſpnœa, and continued fever, are the " very circumſtances which have deterred " phyſicians from uſing the bark. In a " word, theſe are the very reaſons for which " he would immediately give it; it pro- " motes a mild perſpiration, produces a " ſediment in the urine, and diminiſhes the " quickneſs of the pulſe; it removes deli- " rium, by obviating the cauſe or cauſes " which produce the fever, and effectually " removes the breathing *." And again: " Too often the cautious preſcriber waits " for an intermiſſion, and then ventures to " give the bark, when the patient has got " well without it; or while he is employed " in watching nature as it is called, he fre- " quently watches the patient to his grave †." Dr Clark likewiſe informs us, " The cure of " intermittent and remittent fevers in all " their varieties by the Peruvian bark, is " ſimple, efficacious, and the practice uni- " verſally eſtabliſhed. In a former publi- " cation he has ſhown, that there is great " danger in waiting for remiſſions; and that " this

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* Lettſom's Memoirs of the London diſpenſary.

† Ibid.

" this noble febrifuge may be given with " the greateſt ſucceſs, not only in the re" miſſions and exacerbations of fevers in hot " climates, but even when they become " continued."—Dr Clark preſumes, and this I believe with every reaſon, he has had ſufficient experience of the good conſequences ariſing from the like method of treatment in thoſe fevers which uſually happen in Britain *. In his work on the diſeaſes of hot climates, he has given it as his opinion, that, " as ſoon as the inteſtinal " tube has been thoroughly cleanſed, the " principal part of the cure conſiſts in pre" ſcribing the Peruvian bark in as large " doſes as the patient's ſtomach will bear, " without paying any regard to the febrile " remiſſions and exacerbations. If the re" miſſions are diſtinct, the bark will have a " more ſpeedy effect; but even although the " diſeaſe has continued, by its uſe it is " effectually prevented from growing dan" gerous and malignant †." In another part of the ſame book, he obviates an opinion

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* Clark on Fevers.

† Clark on Hot Climates, p. 145.

opinion which prevails againſt the uſe of the bark: " When a fever has diſtinct " remiſſions, few phyſicians will ſcruple to " preſcribe it; but if the diſeaſe aſſume a " continued form, every method is tried to " bring on regular intermiſſions: If this " cannot be accompliſhed, and the patient's " ſtrength begins to ſink, alexipharmics, " bliſters, and cordials, are employed to " ſupport him. The uſe of the bark at that " time would be thought highly dangerous; " and has therefore been cautiouſly prohi- " bited by almoſt every medical writer ſince " the days of Sydenham. But in hot cli- " mates experience affords ſufficient proof " that this objection has no manner of " foundation, and that the bark may not " only be given with the greateſt ſafety, " both in the remiſſions and exacerbations, " but even when the diſeaſe is continual *."

In the fourth volume of the Medical Obſervations we find alſo the following remark: " That whereas moſt writers forbid " the uſe of the bark abſolutely, unleſs in " the ſtate of remiſſion and intermiſſion,

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* Clark on Hot Climates, p. 147.

"I found it neceſſary to give it at any time "of the diſeaſe indiſcriminately; and ſucceſs "juſtified the practice, the patient other- "wiſe diſſolving away irretrievably under "cold colliquative ſweats †."

Another objection to the uſe of ſtimulating remedies is, that they have often been conſidered as increaſing the delirium in fevers; and diſſections have led to the opinion, that in typhus, an actual inflammation had taken place, and given appearance to the fullneſs of the veſſels in the brain. But it is to be obſerved, that this ſeeming inflammation does not depend upon the effect of increaſed excitement in the ſyſtem, but, on the contrary, is entirely owing to a deficiency of it; and as a proof of this, we have in life a ſimilar aſpect. Ophthalmia, in its commencement, is a diſeaſe of a truly phlogiſtic nature; but I believe this ſtate of actual inflammation does not long continue; and if debilitating powers are carried to exceſs, a period enſues, when the diſeaſe becomes paſſive, and inſtead, of admitting of further evacuation, will be in fact cured by tonic and ſtimulating medicines. The redneſs of the veſſels of the eyes in ſcrophulous people, is more eſpecially a diſeaſe of debility; and inſtead of being cured by evacuants, the remedies found to anſwer are thoſe of an invigorating nature. The inflammation obſerved, too, in the gout, I am much convinced, is not of an active nature; but depends on a ſtate of debility, and that ſtimulating remedies here alſo are the cure. The ſcarlet appearance of the fauces remarked in cynanche maligna, affords a ſtriking example of the inflammation I ſpeak of; where it is univerſally known, that the practice is to ſupport the patient ſinking ſo rapidly under this dangerous complaint. Reſpecting delirium in fevers, I apprehend there can be little doubt of its being a ſymptom of debility, and only to be removed by ſtimulants*; and this opinion is confirmed in a letter from Dr Odier of Geneva to Dr Duncan, who not only corroborates what Dr Lind has advanced with regard to the effect of opium when given in

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† Medical Obſervations, Vol. IV.

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* Dr Millar, in making his obſervations on the caſes recorded in his work, comments upon delirium in the following manner. "This ſymptom hath often given "riſe to a ſuſpicion of a phrenitis, as it is called, or "an inflammation of the brain; a diſeaſe juſtly reck-"oned exceedingly dangerous. But the ſymptoms "ought

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“ ought to be very different from thoſe which are generally aſcribed to it. That of a delirium is obſerved in almoſt all remitting fevers, and is only dangerous when aſſailed by copious bleeding and the antiphlogiſtic method of cure. It continues, in maniacs and melancholics, for many years; and ſurely no one would in theſe caſes ſuſpect inflammation. It muſt be confeſſed, that phyſicians have been ſatisfied with very improbable conjectures concerning the diagnoſtics of this dreadful diſeaſe, though their practice hath been as bold as if it were founded on the higheſt degree of certainty. But let us riſe from conjectures to facts. What are the ſymptoms when an inflammation in the brain is occaſioned by external violence? Inſenſibility, coma, and involuntary motions, are the principal ſigns; but a delirium is ſeldom occaſioned by ſuch a cauſe, and is never ſo violent as that which accompanies the remitting fever. Since then it often happens where there can be no inflammation of the brain, and an inflammation may exiſt there without producing ſuch an effect, ſhould we not be extremely cautious of admitting conjectural evidence in oppoſition to eſtabliſhed truths?” See Prevailing Diſeaſes of Great Britain, p. 121. et ſeq.

the hot fit, and which has been already mentioned; but likewiſe informs us, that he has given it with great ſucceſs when there is much delirium, which he never found was increaſed by the exhibition of it, but on the contrary much oftener to be removed, and always to be diminiſhed*.

There is one circumſtance I neglected to have mentioned, and now remark it; it is this: I have repeatedly obſerved the practice of many phyſicians who employ the evacuating modes of cure in fever, ſome of whom conſtantly exhibit nauſeating, nay even vomiting doſes of emetic tartar; and this they employ indiſcriminately, not only in the beginning of the diſeaſe, but at almoſt every period, merely becauſe it is obſerved that the ſtomach is frequently diſpoſed to throw out every thing it contains. The abſurdity of ſuch modes of treatment will appear evident from what I have already delivered. Nay, I will allege, that the ſymptoms of nauſea and vomiting are rendered more diſtreſſing, inſtead of being mitigated

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* Medical Comment, Vol. VI. p. 352.

gated by it *; and not unfrequently, from the uſe of emetic tartar in an advanced ſtate of a fever, the inclination to vomit is ſo much increaſed, as to throw the patient into a deliquium, from which it is with difficulty he can be rouſed, unleſs ſome powerful ſtimulant is inſtantly had recourſe to. And I ſhall recite a paſſage from Dr Clark's work on the diſeaſes of hot climates to ſhew that theſe ſymptoms depend entirely on a loſs of tone in the ſtomach, and not on the effect of cauſes requiring ſuch evacuation. He mentions, "his having found the greateſt "advantage from preſcribing a full doſe of "ſolid opium, when the ſtomach is weak "and ſqueamiſh after evacuation; it ſeldom "failed

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* Dr Gibſon relates the caſe of a gentleman who had a quotidian attended with ſuch an intolerable pain at the ſtomach, that he was of his own accord led to apply a ſmoothing iron ſo hot to the pit of his ſtomach as to bliſter it. Surely this ſymptom indicated evacuation as well as the ſymptoms of nauſea and vomiting; and yet I apprehend a ſingle vomit adminiſtred might have killed him. Dr Gibſon ordered him a conſiderable quantity of bark, a doſe of which he took every hour, and by this remedy he was effectually cured. See Gibſon on fevers, p. 140.

" failed to remove theſe ſymptoms, and
" then the bark will ſit eaſy on almoſt every
" ſtomach. On the contrary, if the diſeaſe
" be allowed to go on, the diſorder at the
" ſtomach will increaſe, and other ſymptoms
" ſupervene, which will render the effect of
" every medicine very precarious *."

From all which, I am of opinion, that the objections to the perſiſting in the uſe of ſtimulating remedies during the different periods of fever are totally groundleſs; and I can only attribute the neglect of theſe medicines to the temperature of this climate, where diſeaſes in general do not make that rapid progreſs they do in warmer countries. Dr Clark ſoon found the neceſſity of laying aſide the evacuating plan, and of having recourſe to thoſe remedies which could alone ſupport his patients ſinking under the effects of extreme debility. And Mr Boone, giving an account of the diſeaſes of Senegal, obſerves, he had near four hundred patients ſo prodigiouſly ill of tertian fevers, that he was obliged to give the bark as common food; and had it not been for this medicine,

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* Clark on Hot Climates, p. 146.

he concludes, we might not have had five men living in the iſland*.

CHAPTER VIII.

CURE OF FEVER, WITH SOME OBSERVATIONS ON PUTREFACTION, &c.

AS I have rejected the ſeveral opinions which have prevailed concerning the knowledge of the proximate cauſe of fever, it neceſſarily follows, that the indications of cure as drawn from theſe hypotheſes cannot be adopted. And having, it is preſumed, rendered it ſufficiently probable, debility is the immediate cauſe of fever, and that every ſymptom immediately depends upon the weakened ſtate of the ſyſtem, the only indication therefore I form, conſiſts in the removing of this ſtate. It may be, however, objected here, that in many caſes of fever, we have only evidence of the application of one power, to wit, that of contagion, and which has been generally conſidered as operating upon the fluids in inducing a putreſcency of them: hence it might be argued, another indication ſhould be admitted of, that of obviating this putreſcency. But I am of opinion, that contagion acts in no other way than as any common debilitating cauſe; and that therefore the tendency which has been ſuppoſed to take place in the fluids to a putreſcent ſtate depends altogether on its effect as ſuch. At the ſame time, I am much inclined to believe that no putrefaction does in fact take place in the circulating fluids during life. As this is a matter, however, of conſiderable importance, and leading to a different practice in many inſtances, I ſhall urge my reaſons for rejecting the generally received opinion that our fluids, in particular diſeaſes, are in a putrid ſtate, which has followed the operation of certain matters acting as ferments in aſſimilating them to their own, and to us a hidden nature, before I proceed to lay down my directions in the employing remedies for the cure of fever.

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* See Appendix to Dr Brockleſby's Obſervations, p. 318.

The firſt argument that would make any impreſſion on the mind of a perſon acquainted with the œconomy of the human frame, is reſpecting thoſe parts which, as immediately requiſite for the ſupport of life, are liable to be affected with diſeaſe. The moſt ſimple view of the human body exhibits a complication of various organs compoſing it, with the degree of ſympathy between each of theſe and the whole, their dependence on one another, with the manner and cauſe of their action: it will therefore be an obvious concluſion, that where the general ſyſtem is impaired by diſeaſe of whatever nature, thoſe particular parts, which poſſeſs the greateſt degree of ſenſibility, and the diſcharge of whoſe functions are indiſpenſably neceſſary to the welfare of the body at large, will be ſubjected on this account to be moſt materially injured. To determine therefore where the ſeat of diſeaſe will be in general, it is requiſite that we take a ſlight view of thoſe parts which more immediately compoſe the body, and of the ſeveral purpoſes which they are deſigned to ſerve. Theſe parts are divided into ſolid and fluid. The former, as made up of many ſmall and innumerable fibres, conſtitute the baſis of the body, and ſerve for the ſeveral motions of it. Theſe motions again are of two kinds, called voluntary and involuntary. The latter is indiſpenſable for the ſupport of life, while the former is by no means ſo: but notwithſtanding, however much they differ in this circumſtance, yet the motions excited in them depend ſtill upon the ſame principle; and it will follow, that the ſtrength and contractility of theſe will be in direct proportion to the number of powers influencing this principle. There are from the beginning of our exiſtence certain powers that conſtantly are acting upon us, and which are unavoidably connected with life; but as their action is by no means confined, ſo it may happen their ſway on the ſyſtem will be variouſly modified upon different occaſions, and which will have the effect of producing diſeaſe. The cauſes inducing this ſtate of diſeaſe in the fibres of the body will depend upon the diminution of the ſeveral powers influencing it; or again, upon the increaſe of theſe. The former effect, as implying a want of the natural and accuſtomed ſtimulus, admits of the fibre becoming lax, and of the coheſion between its particles being deſtroyed, on which the vigour of it ſo directly depends. The other effect is the very reverſe of this, where the tone of the ſolid is unduly increaſed beyond that degree which is compatible with its nature. As whatever ſtate the ſolids of the body are in, whether they poſſeſs their proper vigour, or have this in an increaſed degree; or again, whether they are deprived of it in a great meaſure; it is ſtill certain, that the motions performed by them are owing to the preſence of a principle, whereby it happens, that in conſequence of the application of ſtimuli, it enables the fibres to contract, and endeavour to free themſelves of any power, which if ſuffered to remain might prove deſtructive to it, and which has been called the Vital Principle of Animals.

Having now endeavoured to eſtabliſh that the vital principle of animals is inherent in the fibres of the body, and the injuries theſe may ſuſtain; we ſhall now turn our attention to the conſideration of the fluids of the body, as operating powerfully upon the ſolids to the due performance of their functions. Since the diſcovery of the circulation by Dr Harvey, the attention of phyſicians has been engroſſed with the moſt abſurd theories of the animal machine, attributing the cauſe of diſeaſe to ſome change produced in the fluids, and they have been led conſtantly to conſider is as an hydraulic ſyſtem. On this principle, it will not appear ſtrange, that on many occaſions the ſame remedies were perſcribed to the cure of diſeaſes of the moſt oppoſite caſt, as they were intended to remove particular ſtates of the blood on which the diſeaſe was imagined to be founded. Dr Boerhaave's theory of the Humoral Pathology prevailed very forcibly till within theſe few years, nay, many are ſtill guided by his reaſoning, while the conſtant object of their practice is to remove the ſuppoſed vitiated ſtate of the fluids. But to Dr Hoffman we are firſt indebted for leading us into the inveſtigation of that part of the ſyſtem which is the ſubject of diſeaſe; and we are ſtill in a more particular manner obliged to Dr Cullen, for proſecuting what Hoffman had before ſuggeſted, and rejecting the fanciful, tho abſurd notion of the humoral pathology, when it was ſo lately and ſo fully in vogue. A ſurvey of the formation of the blood from the aliment received into the ſyſtem, together with its uſe, will at once convince us how much the due action of the ſolids of the body is neceſſary to the enjoyment of health; it will give us the moſt ſatisfactory explanation of thoſe parts which are the ſeat of diſeaſe, and will at the ſame time afford us an opportunity to reject the humoral pathology in the caſe of thoſe affections whoſe preſence has been moſt univerſally conſidered as occupying the fluids of the body.

After the reception of the aliment into the ſtomach, (which at the ſame time muſt be in a ſtate of health to admit of the ſenſation of hunger), the ſentient parts, in conſequence of the irritation applied, are ſtimulated to exerciſe their power upon the ſecreting ſyſtem for the digeſtion of it. We ſee evidently then, how eſſential it is to the welfare of the body, that upon taking food, and for the digeſtion of it, the fibres ſhould be in a ſtate fit to diſcharge their functions. It is certain, in the caſe of dyſpepſia, where a loſs of tone prevails over the whole ſyſtem, and particularly in the ſtomach, that though aliment be received into it, yet on account of its debilitated ſtate, the food frequently remains there undigeſted for a conſiderable time; nay, there are numberleſs inſtances where it has been vomited up thirty-ſix or more hours, in nearly as crude a ſtate as when it was firſt received into the ſtomach: And though it may have undergone ſome change towards digeſtion, yet it is undoubtedly certain the chyle is never ſo well prepared, as it would have otherwiſe been if the fibres of the ſtomach poſſeſſed their proper degree of activity. Beſides, Dr Huxham, however much he was wedded to the humoral pathology, obſerves, that in ſuch caſes the nouriſhment is not at all, or not for a long time after, converted into blood.

After the proceſs of digeſtion has advanced a conſiderable length in the ſtomach, the food then paſſes out through the pylorus to be mixed with the other liquors that are poured forth on the inner ſurface of the alimentary canal, which probably have ſtill the further effect of promoting the ſolution of the food, in order to render it more fit to be received into the conſtitution *. And I apprehend it is for this purpoſe that ſuch a quantity of fluids of various kinds are ſecreted, that the aliment may have undergone a proper change before its reception into the ſyſtem; as it is evident, when it comes to be mixed with the maſs of blood, if it contained any hurtful quality, it has then leſs chance of being evacuated. When the chyle has advanced thus far, it then comes to be circulated with the blood, and to be applied to the ſeveral purpoſes of life. Theſe purpoſes are chiefly, to ſtimulate the ſolids to a due performance of their office; and to ſupply the conſtant abraſion going on in the ſyſtem, by depoſiting that nutritious cementing matter which connects the fibres of the body together, giving them that degree of rigidity remarkable in thoſe who are of a vigorous conſtitution.

After this ſhort ſketch of the component parts

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* The various methods of cookery tend perhaps to the ſame end.

parts of the body, and of the chief purpoſes they are meant to ſerve, it is aſtoniſhing that a phyſiologiſt of Dr Cullen's reputation ſhould have conceived a putrid diatheſis of the fluids to be the proximate cauſe of any of thoſe diſeaſes termed Putrid; and his retaining the humoral pathology in one or two inſtances, ſubjects him to be deprived of a part of the great honour and eſteem which he had deſervedly attained for the general rejection of it, as he at the ſame time is well aware that the phenomena of health and of ſickneſs depend upon the ſtate of the primary moving powers; and, though he finds fault very juſtly with Boerhaave and his diſciples for the pathology of the fluids, he yet in the caſe of ſcurvy has laid himſelf open to be attacked on the very ſame ground.

In his preface, in ſpeaking of Boerhaave, he ſays, "That if we conſider the imper-
" fection of his ſyſtem, with reſpect to the
" ſtate and various condition of the animal
" fluids; and if at the ſame time we reflect how
" frequently he and his followers have em-
" ployed the ſuppoſition of an acrimony or
" lentor

" lentor of the fluids, as cauſes of diſeaſe, " and for directing the practice, we muſt, " as I apprehend, be ſatisfied, that his ſyſtem is not only deficient and incomplete, " but fallacious, and apt to miſlead." And again: " Whoever ſhall conſider the almoſt " total neglect of the ſtate of the moving " powers of the animal body, and the prevalence of an hypothetical humoral pathology, ſo conſpicuous in every part of the " Boerhaavian ſyſtem, muſt be convinced " of its great defects, and perceive the neceſſity of attempting one more correct." All this has been applied by Dr Cullen to Boerhaave, which I think is applicable in every reſpect to himſelf; as the reaſons aſſigned by him for retaining the humoral pathology as the proximate cauſe of ſcurvy, are not only vague, but totally void of thoſe circumſtances which ought to form the baſis of a doctrine.

He has given it as his opinion, that the ſtate of debility obſerved in ſcurvy is merely the effect of the vitiated fluids; but how irreconcileable is this with what he delivers in his preface? " That the affections of the " mo-

" motions and moving powers of the ani-
" mal œconomy, muſt certainly be the lead-
" ing inquiry in conſidering the diſeaſes of
" the human body. The inquiry may be
" difficult; but it muſt be attempted, or the
" ſubject muſt be deſerted altogether." I therefore cannot but repeat, how ſtrange it is that Dr Cullen ſhould be guilty of ſuch an abſurdity, by retaining the humoral pathology, when he everywhere condemns the impropriety of that doctrine. Beſides, can it be rendered in any meaſure probable, that the debilitated ſtate of the ſolids, in which the vital principle is inherent, is only a conſequence of the depraved fluids; which, as we have already ſeen, operate only by nouriſhing and giving the ſolids that degree of energy neceſſary for their action, and repairing the waſte of the ſyſtem? And, though Dr Cullen might not have had many opportunities of ſeeing patients labouring under the ſcurvy, yet had he conſulted any of thoſe authors who had repeatedly obſerved it in all its ſtages, as Dr Lind, he would have been ſenſible, that the debility remarked on the attack of ſcurvy, could not be aſſigned with

with any degree of probability to the diſeaſed fluids. For when this diſeaſe firſt ſeizes a perſon, it cannot then be ſuppoſed the fluids have undergone any conſiderable change, (admitting them to be the ſeat of the diſeaſe); every ſymptom at this period, however, plainly demonſtrates that the fibres are and can be alone ſuſceptible of the diſeaſe. The inability to perform the accuſtomed exerciſes, the ſenſe of heavineſs of the extremities, and upon an attempt to perform any motion, yet the feebleneſs of it, and the conſtant effort to lay hold of the moſt trifling objects to ſupport the body, thoroughly evince the impaired ſtate of the fibres. Dr Milman's opinion on this head appears to me perfectly juſt, and carries with it ſufficient conviction that no putreſcency does in reality exiſt, during life, in the fluids. " It is confeſſed by ſome perſons, that the " blood cannot indeed be expected to be in " an actually putrid ſtate, but that it is the " ſtrong diſpoſition of the vital fluid to pu- " tridity which conſtitutes the proximate " cauſe of putrid diſeaſes. This aſſertion " ſeems to me to confound all propriety of

“ language, and every rule of common ſenſe. “ It is to ſay, that an inclination in the fluids “ to putridity, is the proximate cauſe of actually putrid affections. The propenſity “ to any particular ſtate muſt ſurely be “ ſhort of the ſtate to which it tends, and “ incapable of giving that which it has not “ itſelf attained. The eſſential nature of a “ diſeaſe muſt be conſtituted by ſomething “ abſolute; nor is it to be ſought for in “ any future poſſible or probable condition “ of the body. But, though an inclination “ to putridity cannot therefore be conſidered as the proximate cauſe of a corruption “ already exiſting, let us, however, as a “ matter of curioſity, inquire, whether ſuch “ a proneneſs to putridity really takes place “ in the caſe before us or not. If it ſhould “ be ſaid, that the blood of ſcorbutic people “ is not indeed in a putrid ſtate, but in a “ condition nearly approaching it, what is “ the anſwer that might be returned? On “ the authority of experiments, Dr Lind “ aſſures us, that the blood of ſuch perſons, “ kept in the ſame ſtate of the air, did not “ corrupt ſooner than that of healthy people.

“ Thin ſlices of mutton ſteeped in its ſerum continued ſweet, and free from taint, as long as in the ſerum of blood drawn from perſons in health. Whether I conſider, therefore, the nature of the cauſes producing the ſcurvy, or the actual ſtate of the blood in the diſeaſe, or the ſecretions from it in the courſe of the complaint, they all concur to make me believe, that this diſeaſe does not conſiſt in a putridity of the blood. Nor does there appear to be any quality, which can be diſcerned and defined in the blood of ſcorbutic perſons, by which the particular nature of their diſeaſe can be characteriſed *.” And, even admitting that a putrid matter may be abſorbed into the ſyſtem, it is notwithſtanding ſufficiently evident, that thoſe parts which alone are endowed with feeling, can only be made ſenſible of its effect; and to advance that the blood is poſſeſſed of this property, would lead us into the groſſeſt abſurdities: and at the ſame time, if the idea was univerſal, the event might turn out to be the again introducing the once prevailing and univerſal humoral pathology.

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* Vide Milman's Inquiry, p. 57.

The doctrine of aſſimilation has, however, been had recourſe to in this caſe, that when putrid matters are preſent in the fluids, a fermentative proceſs takes place, changing them to their own natures. But this idea is very improbable, and I may add equally impoſſible, that in the conſtant round of circulation ſuch a proceſs could be carried on, when undoubtedly reſt favours it. Dr Ferris, in a diſſertation publiſhed here laſt September, entitled DISSERTATIO PATHOLOGICA, INAUGURALIS, DE SANGUINIS PER CORPUS VIVUM CIRCULANTIS PUTREDINE, has given it as his opinion, that reſt is not eſſential to every fermentation, but does not immediately determine whether the change our aliment undergoes depends on this proceſs *. I ſhall not dwell on this point, whether digeſtion happens in conſequence of a fermentative proceſs, as not being fully determined on, and admitting of doubt: but at the ſame time I would remark, that digeſtion goes on while we remain in the moſt tranquil ſtate; and it is well known in the

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† Diſſertat. Inaug. p. 12. and note.

fermentation of wines, that they reſt perfectly undiſturbed: Nay, a perſon entering a part where a conſiderable fermentation is going on, would undoubtedly be killed, if not very ſoon removed. Beſides, in Portugal and in France, where great quantities of wine are made, provided that motion forwarded the fermentation, they would certainly hurry on the proceſs by ſuch means. But to ſtrictly confine ourſelves to the human body; though a putrid matter was preſent in the fluids, yet Dr Milman advances with the greateſt propriety, that, "In "diſeaſes ariſing from any ſpecific matter, "tho' the noxious particles may be abſorbed, and, floating in the blood, by ſtimu-"lating or weakening may produce effects "on the vital power ſuitable to their pro-"perties, the mixture of the vital fluid is "not much affected. Its ſenſible qualities "are not changed *." This reaſoning of Dr Milman's, however, is oppoſed by Dr Ferris in his Inaugural Diſſertation. He tells us, "Hæc forma loquendi tamen eſt omnino "inepta; eſtque cur miremur Milmannum, "chemiæ

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* Milman's Enquiry, p. 132.

" chemiæ ac aliarum ſcientiarum peritum,
" colorem et qualitates evidentes ſanguinis
" immutatas, pro argumento contra actionem in ſanguinem contagionis propoſuiſſe.
" Contagio extra corpus adeo eſt tenuis et
" ſubtilis, uti oculum, imo imaginationem
" ipſam, effugiat; quapropter vix credibile
" videbitur, eam ſanguini admotam, hujus
" vel colorem, vel qualitates reliquas evidentes, eſſe mutaturam. Namque rubia
" tinctorum, quam omnibus ſenſibus ſubjicere poſſumus, animali ſumpta donec oſſa
" ipſa rubeſcant, ſanguinem ne quidem
" omnino mutat, etſi ejus particulæ per eum
" oſſa ingrediantur oportet. Aſparagus
" ſumptus, ut et terebinthina corporis ſuperficiei admota ac abſorpta, proprium
" odorem tum urinæ cum perſpirationi,
" impertit; aurumque in loculis ægroti, qui
" mercurium devorat, eodem modo, etſi
" lentius, quam ſub mercurio ſubmerſum
" afficitur. Interea tamen qualitates ſanguinis, et evidentes et chemicæ, immutatæ
" manent, nec ullæ particulæ aſparagi, terebinthinæ, vel mercurii ibi deprehendi

"poſſunt *." It appears not a little ſurpriſing a gentleman verſed in the knowledge of the animal œconomy, ſhould have paid ſo little attention in inveſtigating any pathological ſubject, as to conceive, what evidently appears from the above quotation, that the fluids are in a particular manner affected with impreſſions from matters taken into the conſtitution, when he muſt be ſenſible that the phenomena, in every condition of the body, are to be explained only from the ſtate of the primary moving powers. I will not deny, that the fluids are to be conſidered as the moſt eſſential ſtimulus which operates upon the living principle for the diſcharge of its functions; and alſo, from the nature of our œconomy, the fluids may receive the tincture of ſubſtances abſorbed. But at the ſame time I am equally diſpoſed to deny that the ſenſible qualities of the blood can undergo ſuch a change as has been imagined to happen in putrid diſeaſes, and much leſs that this effect follows in conſequence of a putrefactive proceſs. The ſubtility of contagion is undoubtedly of ſuch a nature as

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* Diſſertat. Inaug. p. 14.

as not to come within our cogniſance; but why from this infer our fluids are therefore rendered putrid from its action on the ſyſtem? If it were poſſible that the blood, which is totally deſtitute of ſenſibility, could experience any morbid change, yet thoſe parts which poſſeſſed this in a more eminent degree, and likewiſe being the firſt that receive the impreſſion, muſt moſt aſſuredly undergo the greateſt and moſt eſſential deviation from that ſtate connected with health. Without, however, inſiſting further on this ſubject by reaſoning; if we review the conſequences of expoſure to contagion, we ſhall be perſuaded, that the fibres of the body are the parts which ſuffer from the application of it. The ſhivering that is felt over the whole body, the ſickneſs, anorexia and vomiting, the pain of the head, the wearineſs and inaptitude to exerciſe in a proper degree the neceſſary functions, with the quick weak pulſe, &c. all tend demonſtratively to verify the juſtneſs of my opinion. And to confirm this the more, we may obſerve, there are not wanting caſes, where people having been ſuddenly expoſed to a contagious principle, have had the powers of life ſuſpended for ſome time. And what may be deemed an argumentum crucis, Dr Lettſom informs us of his having attended ſeveral women who laboured under the jail or malignant fever, that gave ſuck, and continued to do ſo through the fever, which he juſtly conſiders as a proof how little the fluids are primarily affected, as the children eſcaped with impunity *.

Another argument I would adduce is, we have no very ſatisfactory accounts of actually putrid blood having been drawn from the veſſels during life; and does it not on the contrary appear, that ſuch a ſtate would be altogether incompatible with our exiſtence for the ſhorteſt time? Would a putrid gore ſtimulate the vital principle in a proper manner for the continuance of life; or could it be ſaid, that the blood is then in a fit condition to repleniſh the waſte of the ſyſtem, or in ſhort adapted for any one purpoſe it ſerves in a healthy ſtate? Moreover,

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* Lettſom's Memoirs of the London Diſpenſary, p. 26.

over, we are informed by Dr Lind, that he let blood from an hundred patients when in the ſcurvy; that the ſerous or watery part perfectly ſeparated from the red concreted maſs; and having examined it, he obſerves, the ſerum was as taſteleſs as the white of an egg; and upon the whole, blood drawn from perſons in the ſcurvy, impreſſes both the taſte and ſmell with the leaſt perceptible ſenſation of any blood he ever put to the trial *. And thoſe authors who mention ſome morbid appearances of the blood † as it flowed from the veins, yet they allow the coagulation of it; but it is very extraordinary, if a putreſcency of the fluids conſti-

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[*] Lind's Poſtſcript on the Scurvy.

[†] Though Dr Rouppe tells us of ſome morbid appearances of the blood, yet he ſays in another part of his work, " That tho' our fluids tend very much " to putrefaction, yet when extravaſated they are not " remarkably putrid, though they ſtagnate in the body " many days; and he is ſurpriſed if there be ſo great a " corruption as often appears from the time they lodge " in a part, that by their mixing with them they do not " change the whole maſs of fluids into a putrid colli- " quation; and yet the contrary is ſeen every day." See Engliſh edition of Diſeaſes of Seamen, p. 317, and 318.

tutes the proximate cauſe of what are termed putrid diſeaſes, that no ſuch diſſolved ſtate ſhould appear in the hundred caſes mentioned by Dr Lind. Neither at the ſame time would the ſeparation of its parts be complete, if theſe diſeaſes conſiſted in a putrid diatheſis of the blood: For the very ideas we would form of putreſcency and of coagulation, are in themſelves as directly oppoſite as we can poſſibly conceive. The former implies a total diſſolution of its parts; while the latter comprehends thoſe phenomena that attend the ſeparation in a healthy ſtate, and which are altogether unconnected with a putrid diſpoſition of the blood.

Having now endeavoured to prove, that the fluids of the body do not in putrid diſeaſes undergo the putreſcency they have been generally imagined; and when of courſe the debility obſerved cannot therefore be attributed to this cauſe, as aſſigned by Dr Cullen; I proceed to take notice of another circumſtance mentioned by him, and which I cannot paſs over. He has ſaid, that "there is hardly an inſtance of ſcurvy ap-

" pearing unleſs where ſalted meats had " been employed, and ſcarcely where the " long continued uſe of theſe did not pro- " duce it." I will venture to affirm Dr Cullen is egregiouſly miſtaken in both theſe particulars. If he had examined Dr Lind's excellent work on the ſcurvy, he would have been ſenſible of his adducing ſeveral inſtances, where ſailors have eat for a conſiderable time of ſalted meats, and notwithſtanding this have eſcaped the diſeaſe. He aſſures us of his having been three months on a cruiſe, during which time none of the ſeamen taſted vegetables or greens of any ſort; and even for a great part of that time, on account of a ſcarcity of freſh water, their ſalted proviſions, which conſiſt of beef and pork, were boiled in the ſea-water; they yet returned into port without a ſingle perſon being affected with the ſcurvy *; and he has a note from Kramer, who affirms, " That ſalted meats have ſometimes no ſhare " in occaſioning the ſcurvy; which is demon- " ſtrable from the many Germans in Hun- " gary deſtroyed by it, who eat neither beef " nor

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* Lind on the Scurvy, p. 51.

“ nor pork; on the contrary, they have freſh “ beef at a very low price *.”

The writer of Lord Anſon’s voyage remarks, it is generally imagined that plenty of freſh proviſions and good water are infallible preventatives of the ſcurvy: at one period, however, the men were ſupplied with both theſe, and almoſt daily caught ſuch a quantity of fiſh, that the whole crew frequently lived on them; yet thoſe, who were ill, were not relieved, or the diſeaſe in any way prevented from following its career †. And when Lord Anſon cruiſed in the Pacific ocean for the ſpace of four months in expectation of falling in with the Acapulco ſhip, the men continued in perfect health; when at another period they were over-run with the ſcurvy in leſs than ſeven weeks after having left the coaſt of Mexico, notwithſtanding of plenty of freſh proviſions and good water ‡.

While the ſcurvy has raged without the pers

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* Lind on the Scurvy, p. 49.

† See Anſon’s voyage in Dr Lind’s book on the Scurvy, p. 446.

‡ Lind on the Scurvy, p. 62.

perſons ſo afflicted having eat of ſalted meats, is evident; it is alſo certain, that the diſeaſe has appeared even among the daily uſe of vegetables, but where they have laboured under the effects of the paſſions, or have been otherwiſe diſagreeably ſituated. Dr Lind has remarked, that two of his Majeſty's ſhips, the Jaſon and Deptford, were employed for a conſiderable time in convoying the merchant veſſels round from Plymouth to the Downs, and calling at Portſmouth in their way. They were rarely longer than forty-eight hours in their paſſage, and eat no oftener than once or twice in a fortnight of ſalted meats for the ſpace of twelve months; and though they were conſtantly furniſhed with ſupplies of freſh beef and greens, yet the companies of both ſhips were much diſtreſſed with the ſcurvy *. And he further mentions, that the ſcurvy made its appearance in the Saliſbury and in the channel fleet, when they had been only ſix weeks at ſea after having left Portſmouth, where plenty of all ſorts of greens had been obtained. And what renders

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* Lind on the Scurvy, p. 273.

ders it unqueſtionable that the diſeaſe was not occaſioned ſolely from the want of vegetables for ſo ſhort a time is, that the ſame ſhip's company of the Saliſbury, when on much longer cruiſes at ſea, have been altogether exempted from the ſymptoms of this diſeaſe, where as to freſh vegetables they were in every reſpect ſimilarly circumſtanced; and it had been noticed that in the longeſt cruiſe that veſſel made, there was only one perſon on board who was attacked with the ſcurvy after having had an intermitting fever †. From which ſeveral inſtances I am warranted to conclude, that Dr Cullen is much miſtaken when he aſſerts, there are hardly any caſes of ſcurvy occurring unleſs where ſalted meats had been employed; and again, where the long continued uſe of theſe did not occaſion the diſeaſe: And as the principles he endeavours to inculcate are therefore found to be void of foundation, it neceſſarily follows, that his reaſoning built on them cannot be admitted.

I have known it urged as a proof of the

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† Lind on the Scurvy, p. 51. and 52.

putrid ſtate of the fluids taking place, that the ſeveral ſymptoms, which conſtitute the advanced ſtate of the ſcurvy, are of ſuch a nature as to leave little doubt in pronouncing from what cauſe they proceeded. The ſymptoms chiefly dwelt on were, the effuſions of blood under the ſkin, with its appearance; the fetid breath, and hæmorrhagies from various parts of the body. But I am confident, that the explanation of theſe phenomena from a lax and debilitated ſtate of the ſolids, will give us more ſatisfaction than our having recourſe to the fluids as inducing them. The appearance of the effuſed blood was deemed a moſt convincing proof of its putrid ſtate: But it is to be obſerved, that the blood collected there is out of the courſe of circulation; and when this happens, it inſtantly becomes liable to undergo the putrefactive proceſs; and hence therefore any particular look the blood puts on in ſuch caſes is by no means to be brought as a concluſion of the affection of the general maſs. Beſides, independent of the coheſion between the particles of the fibres being impaired, how would it happen, that they not only admit of the effuſion of blood under the ſkin, but alſo of the moſt profuſe and dangerous hæmorrhagies? For do not we find, that children, whoſe ſolids are lax, and which do not ſufficiently reſiſt the preſſure of the fluids, are on this account more liable to be attacked with hæmorrhagies than adults? The epiſtaxis affords a proof of this. And again, are not thoſe adults of the ſcrophulous conſtitution more ſubject to hæmoptyſis and phthiſis pulmonalis, th~~an others, whe~~re, inſtead of a laxity or debility of the ſolids, a firmneſs and rigidity prevail?

Since, therefore, it has been rendered probable that the fluids of the body are not in a putrid colliquation when the ſcurvy is preſent, and when of courſe the debility obſerved cannot be aſſigned as the effect of a cauſe which does not in fact exiſt, I would remark, this ſtate of extreme weakneſs conſtantly manifeſts itſelf, and this even from the very firſt attack; whence we ſhould naturally be led to conclude, that ſo prevailing a mark of diſeaſe would moſt likely happen in conſequence of debilitating powers. Beſides, independent of weakneſs, how would it happen that ſymptoms generally called Putrid ſhould occur in the cloſe of a typhus; and more eſpecially too, as we have many inſtances of this diſeaſe appearing, without the perſons ſo affected being expoſed to contagion, and when no alteration can have happened in the fluids from the application of it? In the advanced ſtate of typhus, the ſame putrid ſymptoms preſent as in the ſcurvy, viz. the fetid breath, ſpots of various appearances, and hæmorrhagies, with the ſame gangrenous diſpoſition of the body, particularly in thoſe parts where bliſters have been applied; and yet Dr Cullen does not take into conſideration, in treating of fever, that ſuch a putrid diſpoſition of the blood bears a part of the proximate cauſe of that diſeaſe. Spaſm and atony are the powers which, according to him, induce fever: At the ſame time, however, in reviewing the opinions which have been received on its proximate cauſe, he obſerves, that though the cauſe of fever frequently operates upon the fluids of the body, and particularly occaſions a putreſcency of them, he notwithſtanding maintains, that ſuch a ſtate of the fluids is not commonly the cauſe of fever, and that very often it is an effect only; while in the ſcurvy, he not only makes the putrid diatheſis of the blood to be the proximate cauſe of it, but likewiſe conceives that the debility obſerved is the effect of this depravity. If, therefore, a putrid condition of the vital fluid was the immediate cauſe of the ſcurvy, where the ſame ſymptoms occur in other diſeaſes, the ſame diſſolution ſhould be aſſigned as conſtituting their very natures; but in ſome malignant fevers, the failure of the VIS VITÆ is ſo amazingly quick, as to contradict any notion of their depending upon a putrid diatheſis of the blood, though they be attended with the common marks of putreſcence. And that the ſeveral putrefactive ſymptoms depend upon debility, we may obſerve thoſe perſons who are moſt liable to be attacked with the ſcurvy, and other diſeaſes of debility. They are, in general, thoſe who have ſuffered by former ſickneſs, who lead a ſedentary life, who are enervated by fatigue, who indulge in exceſſes, and more eſpecially thoſe labouring under any dejection of mind. Many inſtances of this laſt have been repeatedly obſerved. The writer of Lord Anſon's voyage remarks, the continuance of the ſquadron at ſea, and the conſequent hardſhips they experienced, were the means of the ſcurvy raging ſo much, that there were but few on board the Centurion who were not in ſome meaſure afflicted with it: And more eſpecially whatever preyed upon the minds of the people, never failed to increaſe the diſorder very much, as it generally hurried thoſe to their graves who had been before confined, and laid up others who had previouſly been capable of diſcharging their office *. And Dr Rouppe, in his treatiſe on the Diſeaſes of Seamen, obſerves, he has known inſtances where the men, expecting to be ſeverely puniſhed for ſome miſdemeanour or other, have been attacked with the ſcurvy ſoon after; and he has related the caſe of a marine,

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* Vide Anſon's voyage in Dr Lind's work on the Scurvy, p. 440 and 441.

who, when the men belonging to the Gorcum were muſtered at Helvoetſluys, begged to be diſmiſſed the ſervice. He then had the ſcurvy; and upon his being aſſured he would be well long before he went aboard, and that he had ſufficient ſtrength of conſtitution to remain long in his preſent ſituation, and that the officers would not permit him to be diſmiſſed, he expreſſed his extreme concern, and ſaid, he would enter, but was certain he would ſoon die: and as he prognoſticated, ſo it proved; for he died the very ſame day.

As it is certain, then, the depreſſing paſſions operate very powerfully in inducing the diſeaſe, and of hurrying it on when preſent, it is likewiſe evident that keeping up the ſpirits of thoſe naturally depreſſed has been attended with the beſt conſequences, as happened in the famous ſiege of Breda, taken notice of by Dr Milman.— Theſe are, therefore, inconteſtable evidences of the ſcurvy being induced, and its having been got the better of, by the effect of the paſſions; and yet it can hardly be preſumed in theſe as well as in many other inſtances which could be enumerated, that the paſſions operated upon the fluids in inducing a putreſcency of them, and again in overcoming that. Surely ſuch an idea is vague in the extreme; for in other diſeaſes, the ſudden change which has been obſerved to happen in conſequence of the effect of the paſſions where putrefaction was altogether out of the queſtion, is a ſufficient proof that no tendency to ſuch a ſtate in any meaſure took place; and I am of opinion it only admits of ſolution, by ſuppoſing that the dejection of mind in ſome manner diminiſhes the excitement of the ſyſtem, as it is undoubtedly certain, that thoſe perſons who are endowed with a ſerenity and cheerfulneſs of mind, are more capable of reſiſting diſeaſe, than others in whom a languor and inactivity prevail.

Dr Lind is of opinion, that whatever tends to impair the conſtitution, and eſpecially the digeſtive organs, may operate ſufficiently, without any other cauſe, to introduce the ſcurvy, in a mild or in a malignant degree, even among ſuch as eat of freſh greens, and who live on the moſt wholeſome diet, and in the pureſt air *. And he informs us, caſes occur, where five or ſix hundred men living on ſalted and on hard meats in a long voyage, are very frequently exempted from the ſlighteſt attack of the ſcurvy; while, upon coming into harbour, and eating of ripe fruits and freſh vetetables, they have been attacked with it in an inveveterate manner. Many hundred ſailors, thus circumſtanced, have been received into Haſlar hoſpital, who have enjoyed good health at ſea; yet they on their arrival have been ſeized with the firſt ſymptoms of this diſeaſe, after partaking of the wholeſome food of beef and greens; and notwithſtanding their being releaſed from the confined ſituation of a ſhip, and the continuance of of a very nouriſhing diet, aided by the adminiſtration of the moſt approved remedies, together with the greateſt care that could be taken of them, the ſcurvy has continued in ſeveral of them for five or ſix weeks †. This effect therefore can chiefly, nay perhaps only, be aſſigned to the irregular way in

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* Lind on the Scurvy, p. 516.

† Ibid. p. 540.

in which ſailors proceed after having arrived from any diſtant part of the world; and I can eaſily conceive, that in the longeſt cruiſes, they may be exempted from the ſlighteſt ſymptoms of the ſcurvy, and even thoſe too who labour under any of the predisſpoſing cauſes; but immediately upon their arrival, that they ſhall be attacked with the moſt inveterate ſcurvy, though their diet be changed infinitely for the better, owing to the many and various exceſſes they are in general guilty of whenever an opportunity offers. And in confirmation of Dr Lind's opinion, Dr Milman has obſerved, that during a ſcarcity of corn, the indigent inhabitants of the colder diſtricts of Italy near the Alps ſuffered conſiderably from the the ſcurvy. Their ſubſiſtence was frequently on the decoctions of a few roots, and often for a whole day they never taſted any food *; which I apprehend ſufficiently controverts any opinion that has or may be formed of the changes the blood can undergo, and of the effect thoſe medicines named antiſeptics have upon the conſtitution in

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* Milman's Inquiry, p. 24.

overcoming a preternaturally ſaline or other condition of that fluid. For that theſe antiſeptics have no very great influence in the cure of the ſcurvy, Dr Lind has informed us, that thoſe patients to whom he gave ſalads of garden-ſtuffs with ripe fruits, &c. did not recover ſooner than thoſe who did not eat them; and it is a fact well known, and mentioned by Dr Milman, that the Turks, though they uſe conſiderable quantities of the vegetable acids in their ſherbet, yet no people are oftener viſited with the plague, or ſuffer more from it than they. I would not altogether, however, aſſert, that the adminiſtration of the juices of fruits, &c. in the ſcurvy and other putrid diſeaſes never was ſerviceable: Yet I am well aware that the commendation, theſe medicines have received, is far beyond that which is their tribute; for are there many, or I may ſay any inſtances, where putrid diſeaſes have been entirely got the better of by ſolely perſiſting in the uſe of theſe remedies? I am of opinion, not: yet practitioners generally commence the cure of theſe affections by their antiſeptics, among which the juices of fruits are ſeldom omitted; but, on account of bad ſymptoms ſupervening, they are under the neceſſity of having recourſe to the Peruvian bark, wine, &c. By theſe means a cure is generally obtained; yet ſo fond are they of the antiſeptic doctrine, they conclude the happy effect is owing to the medicines being antiſeptic. Was this however the caſe, every power according to this quality ſhould be equally ſerviceable; but in many inſtances we know the very reverſe to be the fact. As this matter may receive ſome light by Dr Milman's inferences on thoſe means employed by Dr Lind to try what effect they had in the cure of the ſcurvy, I ſhall inſert what he delivers, in his own words. "All thoſe means of cure "which have been enumerated may be reduced to the following heads: To the "nutrientia, ſudorifica, ſtimulantia, and the "tonica. The firſt of theſe act upon the "ſimple ſolid. By repairing the neceſſary "and daily waſte of the machine, which "could not be replaced by an indigeſtible "diet or inſufficient food, nouriſhment reſtores that ſtate to the fibre, which is re-

" quiſite to the due action of the vital power. " By giving that degree of plethora which is " neceſſary to promote the various ſecretions " and excretions of the body, it prevents that " principle from being weakened by the re- " tention of impure matters, the evacuation " of which leaves it in a condition capable of " properly exerciſing its functions. This is " the manner in which I conceive the freſh " fleſh-broths of the Haſlar hoſpital and the " boiled beef and greens, have rendered the " benefit which has been obſerved to follow " ſometimes their ſeparate, and at others their " joint uſe. The acid juices of fruits, of " the lemons and the oranges, are great pro- " moters of perſpiration and of urine. Their " virtues in the cure of this diſeaſe were " improved by dilution with water-gruel, " which cannot be well conceived to operate " otherwiſe than by giving that fullneſs to " the veſſels which might render the effect " of the acids, as ſudorifics and diuretics, " more certain. But when the diſeaſe was " a good deal advanced, and great weakneſs " had taken place, a pint of rich Malaga " wine, joined with four ounces of theſe

" acid juices, by its ſtimulant cordial properties added great efficacy to theſe; which " was ſtill further increaſed by the addition " of a quantity of ſugar, ſo as to occaſion a " ſort of efferveſcence at the time of taking " this mixture. That the wine, and the " active ſpirit diſcharged by this efferveſcence, are uſeful, by their cordial and ſtimulant qualities, and by concurring with " the acids in gently promoting the proper " excretions of the ſkin and the kidneys, " ſeems evident from the happy preſage " always drawn in this diſeaſe, when the " ſkin, from having been dry, becomes ſoft " and moiſt. Dr Lind moreover inform us, " that the effects of this cordial mixture actually were ſudorific and diuretic. This " view of the ſubject affords us ſome means " of accounting for the beneficial effects " aſcribed to the mineral waters in the ſcurvy. In conſequence of the conſiderable " quantity of fixed air they contain, they " are ſtimulant and diuretic *."

Laſtly, to conclude my obſervations on this head, I would advance, that the remedies

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* Milman's Inquiry, p. 197.

dies and means uſed to reſiſt putrid diſeaſes are thoſe that tend to invigorate the body, ſuch as a nouriſhing diet, the moderate uſe of wine, proper exerciſe, with the article of cleanlineſs. Dr Lind obſerves, that when wine was more freely drank in Holland, the ſcurvy was leſs frequent; and among the firſt cures recommended to the world, was wormwood infuſed in wine; and this was afterwards uſed in Saxony as a preventive, where this diſeaſe occurred very often *.

For any further arguments and facts in ſupport of what I have now delivered, I muſt refer the reader to Drs Lind and Milman's excellent works on this ſubject, as they ſuperſede any thing elſe I can poſſibly offer. Indeed, ſome circumſtances taken notice of have been before remarked by thoſe Gentlemen; but as theſe were ſo cloſely connected with the ſubject, the mention of them was perfectly unavoidable. My chief object was to point out ſome prevailing miſtakes of the ſcurvy hardly ever appearing unleſs where ſalted meats had been employed,

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* Lind on the Scurvy, p. 177.

ed, and where the long-continued uſe of them did not produce the diſeaſe: And having, it is preſumed, ſhown the futility of theſe notions in an evident light, it follows, no particular diſpoſition of the fluids could have taken place, and eſpecially in that caſe where no ſalted proviſions had been made uſe of; which at once muſt overthrow Dr Cullen's idea of the diſeaſe. I have only further to obſerve, that the cauſes inducing ſcurvy and other putrid diſeaſes *, operate upon the ſolids in weakening the vital principle. Every circumſtance corroborates this idea; it gives the moſt ſatisfactory explanation of the ſymptoms; and as the degree of debility increaſes, ſo the different phenomena characteriſing the different ſtages of the diſeaſe enſue; and, to uſe Dr Milman's expreſſion

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* I apprehend the only eſſential difference between the ſcurvy and putrid fevers, is this, that in the former, diſeaſe the vital power is gradually impaired; while in the latter, the cauſes, from their more debilitating effect, haſten the patient ſooner to his grave; and the attack of the diſeaſe, after expoſure to them, in many caſes is ſo immediate, as to refute any idea of the fluids ſuffering any change.

preſſion, the very eſſence of ſcurvy is weakneſs.

I NOW proceed to offer ſome directions in the employing remedies for the cure of fever; and as my only indication conſiſts in obviating the debilitated ſtate of the ſyſtem, the remedies to fulfil this purpoſe are thoſe known by the name of Tonics and Stimulants: under the former of which denominations come the bark, the Columbo and Virginian ſnake-roots, &c.; under the latter, wine, opium, volatile alkali, muſk, and camphire. There are many remedies of the above claſſes that have been uſed in fevers with advantage; but as their effect is generally the ſame, differing only in their degree of tonic and ſtimulant power, my remarks are therefore confined to the Peruvian bark, wine, opium, and muſk. But before proceeding to make particular mention of theſe remedies, I ſhall inquire into the propriety of adminiſtering vomits * in the com-

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* Dr Lind has found a vomit to be attended with the beſt effects on the attack of fever when the patients had

commencement of fever. It appears to me, that on the attack of fever, when for the moſt part a conſiderable quantity of bile is accumulated in the ſtomach; and perhaps at the ſame time a quantity of aliment remaining there in its crude ſtate on account of the loſs of tone in that viſcus, that vomiting may be ſerviceable in removing theſe; and I have no doubt of its proving uſeful by the agitation occaſioned in the ſyſtem, which rouſes the patient from that ſtate of languor marking the attack of fever; and by removing the viſcid matter in the ſtomach, the ſtimulating remedies to be afterwards exhibited may have greater effect by coming more intimately in contact with the ſentient parts. On the contrary, it may be urged, that where an accumulation of matter takes place in the ſtomach, as for inſtance bile, that this is only a ſymptom, depending upon debility; therefore, without any previous vomiting, our attention ſhould

had been expoſed to contagion; and ſoon after its application, they have had a head-ach, with rigors, ſickneſs, &c. which ſymptoms have been entirely removed by the vomit.

ſhould be directed to remove the debilitated ſtate of the ſyſtem, without paying any regard to the aliment, bile, &c. that may be preſent in the ſtomach. That there is good ground for entertaining the idea, I readily admit; yet I have ſeen repeated inſtances, where, on account of the extreme debility prevalent in the ſtomach, the matter lodged proved a diſagreeable irritation to it, inſomuch, that a nauſea and the diſpoſition to vomit conſtantly exiſt: therefore I am of opinion, a gentle emetic, as it evacuates this offending matter, and at the ſame time as the act of vomiting proves ſtimulant to the whole ſyſtem, it may be proper to exhibit it; and that, if immediately after its operation ſome ſtimulating medicine is had recourſe to, the patient, inſtead of being more weakened, will in reality be refreſhed and invigorated by its adminiſtration †. But a ſecond vomit is perfectly inadmiſſible: And when a perſon is attacked with a violent vomiting in the beginning of a fever, no vomit is in this caſe to be given, as this generally depends on an exceedingly debilitated ſtate of the ſtomach and of the whole ſyſtem; while the whole ſtreſs of the cure is to be truſted to the tonic and ſtimulant remedies; and with regard to them, it is to be obſerved, the doſes ſhould be ſmall and frequently repeated, that the virtue of the former doſe may not be gone off before another is exhibited. This was Dr Sydenham's method of proceeding; and I am well convinced it is indiſpenſably neceſſary; for if you give large doſes of any ſtimulating medicine, particularly of a powerful one, and only repeat it at diſtinct intervals, you do more harm than good, by exciting the patient conſiderably, and afterwards ſuffering him to ſink into a ſtate pro-

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† I am only to be underſtood that vomiting is proper on the attack of a fever, and that it is never to be employed in the advanced ſtages of the diſeaſe. The collection of bile, particularly in the warm climates, is ſo quick, that the ſtomach is conſtantly diſpoſed to throw it out, and our utmoſt endeavours prove ſometimes ineffectual

effectual to ſtop the violent retchings. I have been lately aſſured by an able phyſician, that bliſters applied to the pit of the ſtomach have frequently been the means of checking this, and then our medicines will be retained; after which period we have it in our power to correct that ſtate of the ſyſtem, on which the accumulation of the bile depends.

bably lower than what you found him. And further, I would remark, that the tonic are preferable in general to the more powerful ſtimulants; their effect is much more permanent; and on this account there is little danger of the patient ſuffering any great degree of collapſe from the previous excitement: but when the diſeaſe is conſiderably advanced, and death ſupervening, in this caſe the ſtimulants, and thoſe poſſeſſing this power in a more remarkable degree, ſhould be made choice of before others not ſo highly ſtimulant. At the ſame time, the moſt aſſiduous attention is neceſſary in the doſes to be adminiſtered, ſo as to prevent the tendency to death by the debility which ſucceeds the excitement. If this is ſtrictly adhered to, there are few fevers which may not be got the better of; and I have often ſeen and heard of many caſes where the patients have been in very unfavourable circumſtances: yet by perſiſting in the ſtimulant plan, paying at the ſame time proper regard to the directions here laid down, they have ſpeedily recovered; and by continuing in this method, but diminiſhing the doſe as it was found requiſite, any relapſe was moſt effectually prevented. I come now to make mention of the obſervations I propoſed concerning the ſeveral remedies to be employed in the cure of fever; and firſt, of the

BARK. This excellent medicine has long been uſed with conſiderable ſucceſs in the cure of many diſeaſes. We are chiefly indebted to Dr Sydenham for our knowledge of its efficacy in the cure of intermittents. This intelligent phyſician informs us, it had nearly been altogether out of uſe from the improper method of giving it. The mode it was then given in, which with many ſtill prevails, was juſt before the acceſſion of the fit. In this caſe, it ſeldom failed in ſtopping, or at leaſt mitigating, the paroxyſm; yet it commonly returned in the courſe of a fortnight; and on this account it fell into diſrepute, until Dr Sydenham, ſenſible of its virtues, was led not to condemn a medicine merely becauſe it was believed to be prejudicial from a few inſtances having occurred where the patients died after having taken it.

He therefore made further trial of it, adminiſtering it in a more proper manner by giving it in the intermediate days; and the opinion that he had formed of the bark was rendered perfectly juſt from the ſucceſs which attended its exhibition. And here I cannot ſee any juſt ground why the bark ſhould not conſtantly be given in the intermediate days as well as in the day the fit happens. The diſeaſe conſiſts in a ſtate of weakneſs; and the ſooner, ſurely, that this is removed, the patient muſt ſuffer leſs than where it continues for a longer period: we ought therefore to perſiſt in its uſe for ſome time before recourſe is had to other remedies more highly invigorating.

With regard to the doſe, I perfectly agree with Dr Cullen, who is of opinion that bark is only to be depended on when given in large doſes. From the ignorance of many in this, it has often been given, but failed of ſucceſs; when, if the proper quantity had been taken, its operation would probably have produced the deſired effect. Dr Lettſom very juſtly condemns a practice no leſs common than deſtructive to the patient, of giving two ſpoonfuls three times a-day of a weak decoction; he aſſures us, the leaſt that can be depended on in the cure of a fever with urgent ſymptoms of putrefaction is two ounces a-day. This is a quantity, however, that few patients can be brought to take; and though they were diſpoſed to take every doſe, yet in many caſes it induces a nauſea, and in others the ſtomach totally rejects it. This is the leſs to be regretted, as Dr Saunders plainly demonſtrates, that the red Peruvian bark poſſeſſes more powerful effects than the common bark now in uſe: "He ſeldom found it neceſſary to give more of "the red bark than half a drachm every "two hours during the interval of the fit; "and that ſix drachms between the pa- "roxyſms was the utmoſt quantity he ever "found requiſite to be taken; but where the "ſtomach can retain it, a drachm may be "given every hours *." This I think, of the red bark, is a very ſufficient doſe; and with reſpect to the moſt agreeable way of adminiſtering it, it is beſt in the form of

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* Saunders on the Bark.

mixture, with the taſte of it diſguiſed by means of extract of liquorice. There is hardly any one formula, however, that will anſwer in every caſe: ſome perſons can take a quantity of the powder in the mouth, and waſh it over by means of a fluid; while others reject the bark decoction or infuſion, and even the tincture proves diſagreeable. The ſwallowing of the bark in a wafer is a very good way to thoſe who are capable of taking it in this manner.

Having ſaid thus much on the doſe of the bark, and on the moſt proper as well as moſt agreeable form of adminiſtering it, I proceed to obviate an objection that ſome phyſicians entertain againſt its uſe, I mean the notion of its giving riſe to abdominal obſtructions, jaundice, and dropſy; or that theſe obſtructions, when already exiſting, ſhould be cured by evacuants, inſtead of perſiſting in the uſe of the bark and other ſtimulants. Dr Cullen, in the cure of intermittents, lays down two very judicious indications. The firſt is, in the time of intermiſſion to prevent the recurrence of paroxyſms: The other, in the time of the paroxyſms to conduct theſe ſo as to obtain a final ſolution of the diſeaſe. Theſe are indications which are undoubtedly worthy of our attention; but why ſhould he at the ſame time oppoſe himſelf by a third, which defeats his intention in the fulfilling the two former? It is this, the taking off certain circumſtances which might prevent the fulfilling of the two firſt indications. Theſe circumſtances are, a phlogiſtic diatheſis prevailing in the ſyſtem, and congeſtions fixed in the abdominal viſcera. The firſt, we are told, muſt be removed by blood-letting; the ſecond, by vomiting and purging. But Dr Cullen ſurely does not conceive, that when a perſon has been conſiderably exhauſted by diſeaſe, and this diſeaſe too that primarily depended upon the application of debilitating powers, there ſhould be preſent at the ſame time a phlogiſtic diatheſis. I would alledge there was even no phlogiſtic diatheſis before the perſon was expoſed to the debilitating cauſes; for if the phlogiſtic diatheſis was in a proportionable degree to counterbalance that of the ſedative power applied, no diſeaſe would have taken place. But we find that the diſeaſe does actually happen; and when of courſe a ſtate of debility is induced, which no doubt during the diſeaſe is conſiderably augmented, how is it then probable there ſhould exiſt a phlogiſtic diatheſis? Beſides, I would obſerve, there is a ſtrange inconſiſtency in the indications which Dr Cullen has laid down reſpecting one another. The remedies which he employs to fulfil his two firſt indications are thoſe, which, though no phlogiſtic diatheſis was preſent, yet are ſuch as would tend to induce that ſtate in the ſyſtem; and of courſe he is attempting to produce a ſtate of the body on the one hand, which immediately after he intends to obviate; for the phlogiſtic diatheſis, and abdominal obſtructions, are the cauſes which prevent his exhibiting opium.

Reſpecting the abdominal obſtructions, or ague-cakes, as ſome term them, I would apply the ſame argument here as I made uſe of in ſpeaking of the phlogiſtic diatheſis, that, when the patient is reduced to extreme weakneſs, theſe obſtructions ſhould be of ſuch a nature as to indicate evacuation, or that the tonic plan ſhould aggravate the complaint when already exiſting. I am, however, very confident, that though this diſeaſe does depend in a great meaſure on the weakened ſtate of the ſyſtem, yet, on account of the internal local affection, our endeavours to ſubdue it altogether will be for the moſt part fruitleſs: the utmoſt we can do is to palliate ſymptoms; and this, I contend, is to be effected by the uſe of the bark *, &c. Dr Donald Monro, who had frequent opportunities of obſerving this complaint, tells us, " that many practitioners of great repute have been prejudiced " againſt the bark, and inform us, that the " free

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* Sir John Pringle affirms, that the viſceral obſtructions following intermitting and remitting fevers, are not to be attributed to the bark, but to the continuance of the diſeaſe, and to frequent relapſes. Dr Saunders and Dr Millar are both of the ſame opinion. Beſides, Dr Cullen himſelf, in his Materia Medica, aſſerts, that " Bitters may be uſeful in obſtructions of the liver and " ſpleen, as in thoſe of the other abdominal viſcera, and " therefore may be employed in cachectical caſes." See Lectures on the Materia Medica, Dublin 1781, 2d edit. p. 237.

“ free uſe of it often lays the foundation of
“ obſtructions in the abdominal viſcera,
“ eſpecially when it has been given when
“ there was an icteritious colour in the eyes
“ and countenance, and that in ſuch caſes
“ we ought not to give the bark until theſe
“ ſymptoms are gone. At firſt he was very
“ cautious of giving it under ſuch circum-
“ ſtances, till meeting with ſome inſtances
“ where the paroxyſms were ſevere, and be-
“ came more frequent, while the patient
“ was ſo low as to be in danger of ſinking
“ under the diſorder; he then gave the bark
“ freely, as the only remedy capable of pre-
“ ſerving life; which not only ſtopt the
“ ague, but carried off the icteritious ſymp-
“ toms, and reſtored the patient to perfect
“ health.” And he is perſuaded, “ That
“ obſtructions in the abdominal viſcera
“ moſtly ariſe from the obſtinacy of the
“ diſorder, and not from the uſe of the
“ bark; for he has oftener obſerved theſe
“ obſtructions where little or no bark has
“ been uſed, than where it was given free-
“ ly *.

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* Monro's Obſervat.

Mr Cleghorn, too, is of opinion, that although the uſe of the bark has been conſidered as being improper when there is an icteritious colour in the eyes, yet it ought to be given immediately after the appearance of this ſymptom, and that it is dangerous to delay it*. And Dr Lind acquaints us, " That during the ſiege of the " Havannah, while the Engliſh troops ſuf" fered ſo conſiderably by the diſeaſes in" cidental to Europeans in hot climates, the " adminiſtration of the bark was blamed; " becauſe, after the fever had been removed " by its efficacy, the patients were apt to be " afflicted either with the jaundice, a drop" ſy, or a ſwelling and obſtruction in the " liver. But ſuch diſeaſes and obſtructions " of the liver were much leſs dangerous and " fatal than the fever, and were not the ef" fects of the bark, but of the patients con" tinuing in ſo impure an air, and of the " duration of the fever †." " And when the " fever was ſtopt by the bark immediately " after the firſt or ſecond fit, as in my own

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* Cleghorn's Minorca Diſeaſes.

† Lind's Hot Climates, p. 185.

" caſe, and of thoſe of two hundred of my
" patients, neither a jaundice or dropſy enſued; whereas, when the bark could not
" be adminiſtered on account of the imperfect remiſſions of the fever, or when the
" patient had neglected to take it, either a
" dropſy, jaundice, or conſtant headach,
" were the certain conſequences; and the
" degree of violence was in proportion to
" the number of the preceding fits, or to
" the continuance of the fever. By every
" paroxyſm the dropſical ſwellings were viſibly increaſed, and the colour of the ſkin
" rendered of a deeper yellow*."

Another objection to the uſe of the bark is, that when fevers change their type from an intermitting form to that of a continued fever, many phyſicians, under ſuch a circumſtance, reject the bark, and have recourſe to evacuants, with the view of procuring an intermiſſion or remiſſion. That ſuch a practice is totally void of ſupport, will appear from the authorities adduced, when ſpeaking of the proximate cauſe. I would, however, aſk one queſtion of ſuch phyſicians: What occaſions the fever to change from an intermitting or remitting form, to that of a continued? I am much of opinion they will agree with me in ſaying, That it depends on a greater degree of weakneſs being induced: if ſo, then why employ evacuations? Does not the more frequent recurrence of paroxyſms indicate a more violent diſeaſe? I apprehend this cannot be denied me: And if ſo, why not perſiſt in the uſe of the bark? or if this medicine is not found, upon the trial, to ſubdue the complaint, why not then have recourſe to other medicines more invigorating? And that there is not the ſmalleſt ground for the employing debilitating powers, appears from the practice of Dr Sydenham, who did not heſitate to preſcribe the bark when intermitting fevers became continued: neither did he find that wine hurt the patient when adminiſtered with the bark; on the contrary, the heat, thirſt, and other febrile ſymptoms, generally went off, after a ſufficient quantity had been taken. The nearer the intermittent approached to a continued form, he found it the more neceſſary to give a larger quantity of the bark.

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* Lind on Hot Climates, p. 313.

The next remedy that I ſpeak of is

WINE. Wine may perhaps in a great meaſure ſuperſede every other ſtimulant in the cure of fever; and it has this great advantage over all others, of being generally agreeable to the palate. A few inſtances have occurred to me where wine-and-water has been called for by the patient, who drank it with the greateſt avidity. Of all the wines that are preſented to us, I think the red Port anſwers the purpoſe as well as any other; and, with reſpect to the mode of giving it, I am of opinion it ought always to be conjoined with the bark: it prevents the bark from being rejected by the patient, and makes it ſit more eaſy on the ſtomach. If the bark is, however, altogether rejected, we ought in this caſe not to perſiſt in its uſe, but adminiſter in its place a greater proportion of wine. By proceeding in this manner for a day or two, the diſeaſe may in ſome degree be ſubdued; when another trial of the bark ſhould be made, which will now probably be retained by the ſtomach; if not in ſubſtance, a ſtrong decoction or infuſion ſhould be ſubſtituted. When both the bark and wine are given, the doſe of the one ſhould alternate with the other, ſo as to keep up a proper degree of excitement. If the bark be given every hour, for inſtance, half a glaſs of the wine between each doſe of the bark will be for the moſt part a ſufficient quantity; but fully double this ſhould be taken where no bark is exhibited; in ſuch caſes, a bottle in the day will be a tolerable doſe. Our conſtant object, however, which I cannot too much inculcate, is to adminiſter a ſmall quantity at a time, and repeat it frequently, to prevent, if poſſible, any degree of collapſe. As there is for the moſt part a conſiderable thirſt during the fever, ſome weak wine and water, if agreeable to the patient, will anſwer very well for common drink.

The next medicine that comes in ſucceſſion, is OPIUM.

This is a drug poſſeſſing a very powerful degree of ſtimulus, inſomuch that its uſe ſhould be chiefly, if not altogether, confined to thoſe diſeaſes which require ſuch a highly ſtimulant power. I conceive it is ſolely owing to this that writers on the Materia Medica have ranked opium under the claſs of Sedatives: and yet we find theſe very writers everywhere ſpeaking of its ſtimulant effect; and particularly, on this account, they ſtrictly forbid its uſe in inflammatory diſeaſes. With equal propriety might brandy, wine, ether, volatile alkali, and the whole claſs of ſtimulants, be ranked under ſedatives, as opium; for every one of theſe will prove ſedative in their ſecondary operation. It is in this way that exceſs in drinking proves in many caſes a remote cauſe of fever; and yet no one will aſſert, that as long as the ſtimulant effect of the liquor continues, it is a remote cauſe of fever depending upon debility. Many people contend, as a proof of opium being a ſedative, that it can totally extinguiſh life, and that it has the power of inducing ſleep, which likewiſe depends on its effect as ſuch. Reſpecting the firſt of theſe arguments, I would allege, that brandy, ether, and many other highly ſtimulant powers, will produce the ſame effect if taken in ſufficient quantity; for it is to be obſerved, that opium only deſtroys life by an indirect operation; when its primary operation is over, the collapſe enſues: and as a proof of this we find, that patients labouring under any inflammatory complaint never die during the inflammatory ſtage; it is only after this ſtate when the debility takes place in conſequence of the excitement, that they run any riſk; to prevent which, during the inflammatory diatheſis, we muſt have recourſe to evacuations.

And, with regard to the latter argument, I would aſſert, that opium, in many caſes, inſtead of procuring ſleep, prevents it by its ſtimulant effect. If a perſon has an inflammatory diſeaſe, and gets a doſe of opium, I will affirm, that it will increaſe every ſymptom of the inflammation conſiderably; and, inſtead of inducing ſleep, it will have the contrary effect, of making the patient watchful. A very remarkable caſe of this nature came within my own knowledge. A gentleman laboured under mania, with the moſt furious delirium and pervigilium. The phyſician that attended, a man of eminence in his profeſſion, very properly preſcribed venæſection, and likewiſe ordered a pretty ſmart purge. According to his direction he was bled, and on the ſame morning he got the cathartic. The conſequence of the evacuations was, that from being delirious in the morning, towards the evening his ſenſes were infinitely more perfect, and he was inclined to ſleep. But moſt unfortunately the people of the houſe with whom he lodged, gave him about 40 drops of laudanum, thinking it would favour his inclination to ſleep. It had however the moſt directly oppoſite effect. The pervigilium returned, with the redneſs and fluſhing of the face; and in ſhort, by its adminiſtration, every ſymptom was grievouſly aggravated. I would even allege, that opium given to perſons in perfect health at bedtime, will prevent, inſtead of occaſioning ſleep, until its ſtimulant effect is over, as I have frequently experienced. A toſſing and tumbling were the certain conſequences of my taking a draught compoſed of forty drops of laudanum immediately on getting into bed. Therefore, when opium is preſcribed to a patient, perhaps with only a ſlight indiſpoſition, and when it is meant to induce ſleep, it ought to be taken an hour or two before going to bed.

Another proof of opium being highly ſtimulant is the loquacity and cheerfulneſs which it produces even among thoſe whoſe ſpirits are naturally depreſſed; and if taken in ſufficient quantity, it can occaſion intoxication. Dr Young informs us of a ſurgeon who always made it a point of taking a doſe of opium before he performed any conſiderable operation, to ſupport him under it*. There is a very ſtriking example proving the ſtimulant effect of opium in the caſe of a hypochondrical patient, related in the 3d volume of the Phyſical and Literary Eſſays. Having read in ſome magazine or newſpaper of the quantity of opium taken by the Turks, and of the invigorating effect it had upon them, he was led to procure ſome, and of which he took a very conſiderable quantity. The conſequence was, that during its ſtimulant

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* Young on Opium.

effect he experienced the utmoſt ſerenity and chearfulneſs, and he found himſelf in greater ſpirits and vigour than he had been ſenſible of for a conſiderable time before; but when its ſtimulating power had ceaſed to act, and a ſtate of collapſe enſued, he complained of a violent ſickneſs, and fancied he ſaw frightful ſpectres, and in ſhort he was in the utmoſt uneaſineſs. He however got better of theſe ſymptoms, and expreſſed his ſtrong deſire for another doſe, having experienced the moſt inexpreſſible ſatiſfaction for ſome time. The moſt ample proof, however, which we have of the effect of opium, is in the Turks, who are aſtoniſhingly addicted to it. They are ſenſible of its giving them a flow of ſpirits; and when they once begin to take it, a habit is ſoon acquired, and to produce the ſame effect the doſe muſt always be augmented, and on their going to battle the quantity they take is almoſt incredible.

Taking it, therefore, for granted, that opium is poſſeſſed of a conſiderable ſtimulant effect, I proceed in the next place to obſerve, as in many caſes the bark runs off by ſtool, though given in any form, that in order to obviate this a mixture with eighty or ninety drops of laudanum ſhould be preſcribed, of which the patient may take a ſpoonful or two every hour. There are alſo other caſes which require the uſe of opium. Many intermittents prove ſo obſtinate as not to be cured by the bark; and though we were to perſiſt in its uſe we ſhould not ſtop its progreſs, and the ſituation of the patient would become dangerous. Hence medicines of a more invigorating nature become neceſſary. Dr Donald Monro obſerves, that the bark was found in general the beſt remedy; yet it did not anſwer in every caſe, and when the patient ſunk under its adminiſtration, other medicines were given which had the deſired effect.

Dr Odier of Geneva remarks in a letter to Dr Duncan, "of his having ſeen a caſe "in St Thomas's hoſpital in London, in "which Dr Fordyce gave a doſe of Dover's "powder, with a ſweating draught of ſalt of "hartſhorn, two hours before the fit, in order "to ſtop it. This ſucceeded very well. A "profuſe ſweat came on juſt before the

" hour the cold fit uſed to return, and it " was altogether prevented. Some doſes of " Peruvian bark were afterwards admini" ſtered; and this fever, which had before " reſiſted many remedies, even the bark it" ſelf, was then in a few days effectually " cured. This obſervation we have had " many opportunities of repeating here; " not only in the quartan, but alſo in ter" tian agues; and we find, that wherever " the patient can be brought to ſweat, the " fit is altogether ſtopt. If he does not " ſweat, the fit is nevertheleſs much dimi" niſhed thereby*." In ſpeaking of the proximate cauſe of fever, I obſerved the effect of opium and bark in exciting ſweat, with the diminiſhed heat of the body that took place; and likewiſe attempted the explanation of this circumſtance. I ſhall not here make any comment on what I before delivered, further than that in almoſt every caſe of fever where I have read of ſtimulating remedies being given, I conſtantly have found that a profuſe ſweat was the neverfailing effect which followed their exhibition.

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* Sixth Vol. of the Medical Comment.

And I would remark, that the ſweating which follows the uſe of antimonials never becomes near ſo univerſal as that procured by opium, &c. though to favour the operation of the antimonial, the patient is generally loaded with a heap of bedcloaths, and probably is deſired to put on a flannel ſhirt; and notwithſtanding this, the ſweating is for the moſt part extremely partial, appearing only in thoſe places neareſt the heart, the extremities being nearly void of it; while as Dr Lind obſerves, there is an agreeable ſoftneſs of the ſkin in thoſe who are ſweating from the uſe of opium, inſtead of the burning ſenſation which affects perſons ſweating in the hot fit, and who are not under its influence.

Under the article of Musk, I have very little to offer, as opium in every caſe may ſuperſede its uſe. There can be little doubt, however, of its having proved to be of great efficacy in removing hiccup and ſubſultus tendinum, the conſequence of advanced ſtages of weakneſs. Dr Clark obſerves, that "muſk given to the quantity of a ſcruple

" every four hours, ſeldom failed to abate " the hiccup and other convulſive ſymp- " toms attendant on fever, and to act as a " powerful cordial and diaphoretic*." The great objection to muſk is, we ſo ſeldom get it genuine, that we cannot depend much on the effect which the doſe may have, and at the ſame time the extravagant price it bears, renders it a medicine not adapted to the circumſtances of every patient. When muſk is notwithſtanding preferred to opium, it is ſtill to be obſerved, that it only becomes neceſſary when the diſeaſe is far advanced, and the patient ſtands in need of ſuch a cordial: therefore the firſt doſe ſhould be pretty large, about the quantity of half a drachm made into a bolus; and after this, ſmaller doſes ſhould be ſubſtituted; carefully preventing any collapſe, by giving a ſecond doſe before the effect of the firſt be perfectly worn off.

Having now finiſhed the remarks I propoſed concerning the remedies to be employed in fevers, I have only to obſerve, that bliſters may occaſionally be had recourſe to. When a fever is conſiderably advanced, attended with a quick fluttering pulſe and coma, I am of opinion the application of a bliſter will have a good effect. In two caſes I remember to have treated, the patients were in a very bad ſituation, having frequent pickings at the bedcloaths and ſubſultus tendinum, I ordered a bliſter to be applied to their heads in the evening; and next morning I was not a little ſurpriſed to find them conſiderably better, when I expected every hour would have been their laſt. The pulſe in both, which had been ſo feeble and quick in the evening as hardly to be counted, was now much diminiſhed in quickneſs, and had acquired a greater firmneſs. This effect I was pretty certain was owing to the bliſter; for they had not taken any thing during the night. Nor do I think it ſo aſtoniſhing as I then did, as Dr Lind has found the application of them followed with an excellent effect. He has ordered patients in the evening to be bliſtered, and has left them with pain and confuſion of the head, with a quick pulſe, great heat, immoderate thirſt, and the eye has exhibited ſuch a dejected look as to portend imminent danger. But next morning he has found them infinitely better, their eyes have aſſumed a more natural caſt, the pulſe has been diminiſhed in quickneſs, and their general appearance has beſpoke a nearer return to health *. Bliſters are, however, only to be employed in the advanced ſtages of fever; and then their application is to be conſidered merely as adjuvants, as we at the ſame time adminiſter our ſtimulants internally. Air of a moderate temperature may alſo be employed: it for the moſt part proves very refreſhing to the patient; prevents the accumulation of the effluvia from his body, which as a cauſe of many fevers will no doubt aggravate the the diſeaſe when already exiſting, and therefore ſhould be ſtudiouſly avoided: and if the patient's ſtrength will permit, he might be taken out of bed; but it is requiſite he ſhould be placed in a horizontal poſture, to prevent the riſk of his falling into a faint.

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* Clark on Hot Climates, p. 142.

The PRACTICE here recommended is applicable

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* Lind on Fevers and Infection.

plicable to the cure of intermittents as well as to fevers of the continued type; and from a thorough conviction of its propriety and neceſſity, I truſt that the arguments and facts brought in ſupport of it will be maturely conſidered before they are deemed unavailing.

N. B. The Author flatters himſelf, any inaccuracies in the ſtyle, with which he is certain this performance abounds, the candid reader will readily overlook.

FINIS.

ERRATA.

Page 8. line 5. for *has*, read *have.*
P. 38. line 6. for *evinces*, read *evince*.
P. 62. line 6. inſtead of *involuntary excretions*, read *involuntary paſſing of the excretions*.
P. 64. line 21. for *depend* read *depends*.
P. 95. line 20. for *are*, read *is*.
P. 120. line 14. for *are*, read *is*.

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